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EDITORIAL.

ITEMS AND NOVELTIES.

Mineral Cotton.—At the last meeting of the Franklin Institute Mr. Coleman Sellers exhibited a sample of a material which we believe is now for the first time about to be manufactured and applied to useful purposes in the arts.

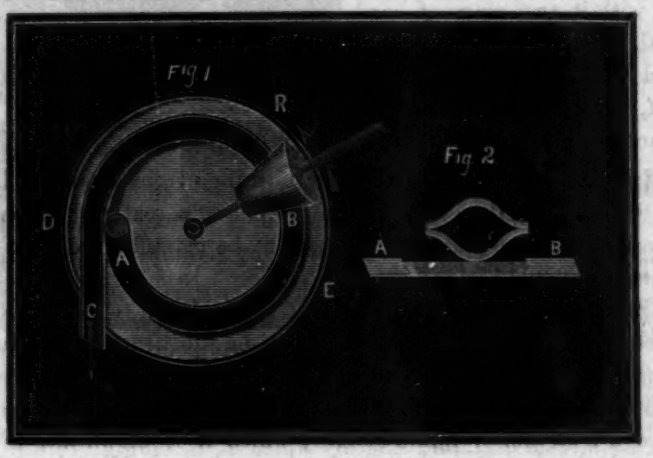
The product possesses a general resemblance to cotton wool, for which it may doubtless in certain cases be substituted with advantage, but on closer examination seems more like a spun glass, which in reality it is. It is formed by allowing a jet of steam to escape through a stream of liquid slag, by which it is blown into the finest threads, sometimes two or three feet in length. These threads, though somewhat elastic, readily break up into much smaller ones, and, the color of the substance being white, the appearance of a compacted mass of it makes the name under which it has been described a very appropriate one. The admirable non-conducting property of the material for heat, as well as that of the great quantity of air which it retains in its interstices, would seem to fit it very well for a non-conducting casing to steam-boilers and pipes, an application for which it is at present being tested.

The Business of the Suez Canal.—From the tabulation of the Austrian Consul at Port Saïd it appears that the number of vessels which passed through the canal in the direction of Suez during the year just passed amounted to 292, while in the opposite direction 199 made the passage. Representing the nationality, the 491 vessels would be divided as follows:

English,	314	Spanish,	3
French,	74	Dutch,	3
Egyptian,	33	American,	2
Austrian,	26	Russian,	2
Turkish,	18	Danish,	1
Italian,	10	Greek,	1
Portugese,	3	Zanzibar,	1

The total receipts of the canal company amounted to 5,070,093 fcs.

A Peristaltic Machine.—That is, a simple water-pump, "in



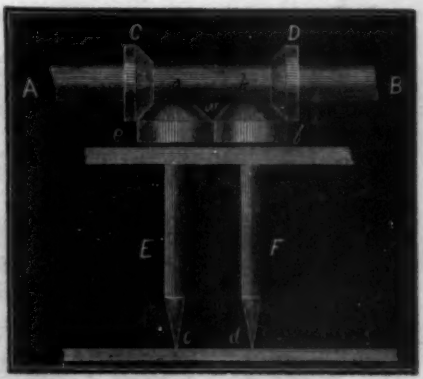
which A B C, fig. 1, is a spiral tube, duly fastened to the bottom of a shallow tub, D E. At B is seen a conical roller, having the middle of the bottom of the tub for its summit and center of motion. The tube A B C occupies rather more than one circumference, so that the cone presses, during a small part of its revolution. on both spires at once, by which means the machine *would act* without even one valve, though it is better to place one *under* the opening, A. Now observe the operation as the cone rolls over the tube, round the common center, in the direction of the arrow, R. The water enters behind it,

through the opening, A (for the tube is plunged a few inches into the water), and is forced by its pressure into the ascending tube, which is a continuation of A B C.

"It would be superfluous to add that these tubes are shown in the figure *as cut open*, and presenting their inside to view."

"An objection may occur to some, at sight of this machine, namely, that the roller or cone, B, would soon destroy the flexible tubes, by their excessive pressure. But, to obviate this diffficulty, I have added, in fig. 2, a form of tube (supposed of leather) which insures a proper *position* of the tube under the roller, accompanied by ledges A B, on which their surplus weight would lean, so as to annul every excess of pressure on the tube."—*Century of Inventions.* James White. 1822. J. H. C.

Anti-friction Device.—"A B is an axis which it is desirable to divest of its *friction*. To do this, as nearly as may be, I connect with it two rings of hard metal, C D, formed as truncated cones, and under the shaft, in the same vertical plane, I place two smaller shafts, E F, carrying on their tops other two cones, similar to the former.



"The summits of each pair of cones meet, of course, in the points *a b* of the main shaft; and, on the principle of bevel-gear, every contiguous part of the touching cones moves with the same velocity, so that there is no sensible *rubbing* between them; for, 1st, the pivots *c d* are hard and pointed, and run on the hardest *steps* that can be obtained; and, 2dly, the tendency of the cones, *u*, towards each other is repelled without friction by the cylinders, *e f*, attached to them, and which *lean* right and left against each other, turning with the same velocity, without causing any friction or any *creeping* between the two pairs of cones *e* C and *f* D. All the weight, therefore, of the shaft A B (which, of course, is kept in place in the other direction by proper side checks, &c.), rests on the points of the vertical shafts E F, accompanied by no sensible tendency of these points to quit the places assigned to them."—*Century of Inventions.* James White. 1822. J. H. C.

The Comparative Efficiency of Boiler Plates.*—Messrs. Whelpley & Storer communicate to a contemporary the results of tests made with nine different brands of boiler plates, with reference to the determination of their heat transmitting and steam generating efficiency. The estimation of the results was made from the relative generation of steam under similar conditions. The thickness of the plates was uniform ($\frac{5}{16}$ of an inch); the temperature of the flame varied but a trifle from 550° Fahr., and the time of evaporation of water but a few seconds.

Allowing the plate of lowest transmitting power to have the value 100, the relative efficiency of the others will be indicated in the following tabulation:

1	Power of Transmission,	. . . .	100
2	"	"	104·4
3	"	"	117·7
4	"	"	118·8
5	"	"	121
6	"	"	123
7	"	"	123·3
8	"	"	141·9
9	"	"	144

To generate an equal amount of steam, in equal times and under similar conditions of fuel and draft, boilers made of Nos. 8 and 9 plates would consume constantly 40 per ct. less than boilers made of plates Nos. 1 and 2.

The authors very sensibly conclude by directing attention to the vastly greater efficiency of the best qualities of plate in the generation of steam. The possibility of a saving of 40 per ct of fuel is certainly an inducement to purchase the best plates.

Testing of Boilers.—A really useful and positively determinative test of competing boilers has taken place at the Fair of the American Institute, under the direction of the Committee on Engines and Boilers, Prof. R. H. Thurston (*Stevens' Institute of Technology*), chairman, T. J. Sloan and R. Weir, members.

Instead of weighing the water fed into the boiler and the fuel consumed, and reporting their ratio as the evaporative efficiency of the boiler, a method has been adopted that enables the committee to report the real evaporation and also the amount of water carried over, unevaporated, by the steam.

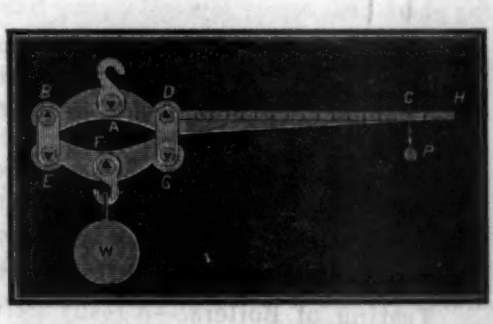
* Sci. Amer. xxv, 324.

A large surface condenser has been prepared, of about 1100 square feet of condensing surface. The steam from the boiler is blown off at a pressure of 75 pounds into this condenser, and the *quantity of heat* carried over by it is measured in thermal units. Should this amount be greater than would be transferred by saturated steam, it shows the steam to have been superheated; and if the amount is less than would have been necessary to fully evaporate all water passing out of the boiler, it indicates that the steam was wet, and a simple calculation gives in each case the exact weights of steam and of water passing into the condenser.

The quantity of feed and of injection water is measured by meters, the water of condensation by weighing, and the temperatures of steam, feed, injection water, water of condensation, and of discharge water from condenser, are carefully taken, and the temperature of the gases in the flues is taken by a pyrometer.

The results are reported to be very satisfactory, both as a test of this method and of the boilers competing. We shall obtain the results as soon as they are made public. They will be exceedingly interesting to engineers and to the public.

A Differential Steelyard.—"Suppose the arm, A C, to be 4 feet in length, and the difference between A B and A D $\frac{1}{10}$th of an inch, and E G equal to B D, and E F and F G equal, then the power of the weight, P, to raise the load, W, is as 48 inches to $\frac{1}{10}$th of an inch, or as 480 to 1; so that if the weight P were 10 lbs. this steelyard would weigh 4800 lbs.; and it is easy to see that this power can be almost indefinitely extended."—*Century of Inventions.* James White. 1822.



J. H. C.

A New Street Lamp.*—Mr. Skelton has invented a plan of utilizing much more of the light than is usually economized in lamps and lanterns. It is evident that the rays of light which enter the eye of a spectator on the ground form only a fraction of those given

* Jour. App. Chem., vi, 172, from Nature.

out by the flame. Those passing through the upper portion of the sides and roof are either entirely lost or, at least, but very imperfectly reflected by clouds, &c., and become visible only in the glow which hangs over a distant city. The principle of the inventor is to apply a number of reflectors, in such positions as to bend downwards and utilize the light now wasted by upward radiation. The upper half of each side of the lamp, and all of the sloping sides of the roof, are occupied by a frame in which strips of looking-glass are arranged with their reflecting surfaces downwards, in a manner analogous to the laths of a Venetian blind. The plan is applicable to any form of lamp, and cannot fail to prove serviceable.

Phosphorus Bronze.—Some recent experiments, made for a committee of the French Academy, upon the properties and the merits of the application of the new alloys containing phosphorus, seem to have resulted very favorably. One of these experiments included a bursting trial of a six-pounder cannon of phosphorus bronze, compared with a similar gun in ordinary bronze cast at the royal foundry at Liège; the result showing that under the bursting charges the regulation piece had burst, while the new gun could still be fired with perfect safety. The bronze employed was made by adding phosphorus copper to metal coming from old guns.

From the great hardness, toughness and stability of the new compounds, as evinced by these as well as other enumerated trials, it is safe to conclude that their future applications will be very numerous. As an example of the application of the bronze in machinery, it is stated that a pair of pinions of universal rollers had been used for ten months, and were finally destroyed by the wearing away of the teeth, none of them having been broken or split. It has also been successfully applied, according to our account, for the collars of hydraulic presses, eccentric rings for locomotives, pistons and bolts for steam cylinders, etc.

Dualin.—A series of experiments have recently been made, in Lancaster County, Pa., with the, as yet, comparatively little known explosive called Dualin. The material is described as being in the form of a powder, resembling fine saw-dust coated with oil. It is declared to possess an explosive capacity far exceeding that of nitroglycerin, and to be withal as safe to transport or to manipulate as gunpowder. The only method of discharging it explosively is said to be by concussion. Passing over the details of the drill blasting,

which were quite satisfactory, we append the most remarkable feature of the trial—the account of the surface blasting:

"On a detached rock weighing about six tons was placed a carriage containing two pounds of the compound. This was covered with half a bucketful of sand, and trampled down with the feet. The end of a fuse was inserted in a copper cap, a little larger than an ordinary gun cap, and the fuse ignited. . . . The rock was broken into fragments, and so disintegrated that particles taken between the fingers crumbled like dried clay."

The non-explosive character of the compound, save with the aid of a fulminator, was demonstrated by elevating with a derrick, to the height of sixty feet, a keg of the material, and allowing it to fall upon a face of rock, which experiment was not followed by a discharge.

An Electric Pyrometer.—The plan is suggested by a German *savant*, of applying the differences of resistance to the galvanic current, evinced by metals upon being subjected to changes of temperature, to the measurement of temperatures when other forms of pyrometers become unreliable.

The resistance of a wire of any metal, at a given temperature, is a measurable factor; and, in the plan above mentioned, a wire of platinum whose resistance has been determined, is placed between two cylinders of clay, and connected with a battery and with a measurer of resistance. The arrangement then is subjected to the heat to be measured.

A New Respirator.—In a recent lecture at the Royal Institution of Great Britain, Prof. Tyndall describes a new respirator, designed for the use of firemen and others for protection against smoke and the noxious vapors evolved during combustion. In the words of the lecturer, the object of the contrivance is to place it in our power to penetrate through the densest smoke into the recesses of a house, and to rescue those who would otherwise be suffocated or burnt. As constructed, after a number of experiments, it consists of a number of strata separated from each other by wire gauze. On the partition of wire gauze at the bottom of the space which fronts the mouth, is placed a layer of cotton-wool, moistened with glycerin; then a third layer of dry wool; then a layer of charcoal fragments; a second thin layer of dry cotton-wool, succeeded by a layer of fragments of caustic lime. The order of succession of the layers is unessential, and may be changed without interfering with the action of the instrument. A wire gauze

cover keeps the substances in place. The moistened cotton is designed effectually to arrest, by its adhesive action, the carbon particles constituting the smoke; and the charcoal, by its surface action, to condense the more dangerous and irritating vapors, mainly hydro-carbons, produced by imperfect combustion. The lime is for the absorption of carbonic acid; but, in an atmosphere filled with smoke, this substance is never present in dangerous quantity, and this last layer may therefore, except in special cases (in wells, mines, &c.), be safely dispensed with.

This instrument, furnished with a suitable hood, has been thoroughly tested by the Engineer of the London Fire Department, and found completely to answer the purpose of its designer.

A New Photometric Unit.*—Prof. John C. Draper proposes a new unit for photometric purposes, which, besides being theoretically much more scientific than that at present commonly used, offers no difficulties for its adoption in practice. The method of Bunsen is that usually employed, and depends upon the determination of the relative intensities of two lights, one of which is supposed to be invariable. The invariable unit in this method is the light produced by the burning of a candle; which is defined as a sperm candle of six to the pound burning at the rate of 120 grains per minute.

It will be evident, upon a little reflection, that this plan is liable to serious errors in practice, from the fact that candles can never be obtained possessing the same composition, from which it follows that the light emitted in a given time by two candles with the same consumption by weight, will be a variable, and not as the theory demands, a constant factor.

The plan suggested by Prof. Draper avoids this error completely; and consists in the adoption of the light emitted by an incandescent solid—a platinum coil—heated to a certain temperature. With a given substance, a light of a definite and measurable intensity is emitted at different temperatures. The invariability of the light from this source, at any convenient degree of heat, affords a satisfactory unit; it is only necessary that the flame employed to heat the coil shall be itself non-luminous.

The course adopted by the author is to allow a flame of pure dry hydrogen, burning at a definite rate, to impinge upon a platinum coil; and it was found on experiment, that so long as wire of the same

* Sci. Amer., October, 1871.

diameter was used in constructing coils of the same dimensions, and those were subjected to a flame of hydrogen burning at the same rate, the light emitted by the glowing wire was of the same intensity.

There can be no doubt that measurements made upon this method will be far more reliable than those made in the usual way.

The Spectrum of the Aurora. — Prof. Geo. F. Barker. Through the kindness of the author, we are in receipt of advanced sheets of the *Am. Journal of Science*, containing the results of his observation of the beautiful crimson aurora of November 9th, a fine display of which was visible at this place. The observation resulted in one point of great interest, namely, the detection of a line in the auroral spectrum not hitherto observed.

The following tabulation of the observation, with the accompanying explanation, will suffice to present the essential portion of the paper:

Line	Scale Number	Wave Length	Auroral Lines	Other measurements.
B	76	687		
C	82	656		
(1)	90	623	623	627 Zöllner
D	100	589		
(2)	110·5	562	562	557 Angström
E	124·5	527		
(3)	130	517	517	520 Winlock
b	130	517		
(4)	138	502	502	
F	146·5	486		
(5)	145	482	482	485 Alvan Clark, Jr.
G	189	431		

In this table, column 1 gives the auroral and the Fraunhofer lines; column 2, the number of these as measured upon the scale of the spectroscope used; column 3, the wave lengths of these lines; column 4, the wave lengths of the auroral lines; and column 5, the wave lengths of auroral lines noticed and measured by other observers, and which the author assumes to be identical with those opposite to them in his own observation. It will be seen that the line (4), of wave length 502 is a new one, since it appears not to have been noticed by former observers.*

*In a note appended to the above, Professor Barker mentions that the last number of the *Astronomische Nachrichten* (No. 1864), received since his notice was in type, contains an account by Vogel of the spectra of auroras seen at

The Utah Tin Discovery.—For some time past the newspapers of the country, have extensively circulated the report of the discovery of vast deposits of tin in Utah, which were declared to exceed in extent anything of the kind at present known. Samples of the ore, which had been sent to Washington for assay, are said to have been returned with the announcement that they contained on an average 37 per cent. of metal. Those who have been interested in originating and, perhaps still more so, in circulating this story of the "tin mountain," and especially those who are fortunate enough to possess stock in the company now formed to work it, will probably feel some surprise that so eminent a chemist as Dr. F. A. Genth has had the misfortune to find "not a trace of tin" in the samples submitted to him for analysis. The appended extract of a letter from Dr. G. to the Editor of the *U. S. R.R. and Mining Register*, will need no comment.

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"These are certainly wonderful discoveries, and judging from the character of the "ore," there can be no doubt that it exists in vast quantities. It was my good fortune already over one month ago to receive some of that which had been sent to Washington. About fourteen days ago I received a second lot for examination, and was also favored with a visit from one of the owners, who brought larger lumps and showed some bars of tin which had been melted from the ore, and also some copper which had been tinned with the product of such smelting operations. My specimens are *undoubtedly authentic*. They consist of a rock, composed of white feldspar (probably albite), hornblende and a small quantity of quartz. The albite and hornblende are present in variable quantities, sometimes the one, sometimes the other predominating. Ocular inspection did not show a trace of *tin*; concentration of the heavier portions, by grinding the rock and washing off the lighter, and the chemical examination of the heaviest did also show not a trace of *tin*. A very careful analysis and a crucible assay showed likewise the total absence of *tin*. The specimens which I received are, therefore, *no tin ore* at all, but syenite —a granite, in which mica is replaced by hornblende."

the Bothkamp observatory. White auroras gave five lines, red ones, seven. Among these was one of wave length 500·3, which Professor Barker says is apparently the same as that seen by him of wave length 502. In this note is given a table of all the lines hitherto observed and measured in the aurora. They number eleven in all, though no single observer has seen more than seven at once.

Coating with Zinc in the Wet Way.—According to M. Böttger, copper or brass may be given a firmly adherent zinc coating, by the following method:

Finely divided or powered zinc, in a non-metallic vessel, is covered with a concentrated sal-ammoniac solution; this is heated to boiling, and the articles of copper or brass, properly cleansed, are introduced. A few minutes suffices to produce a firm and brilliant coating. The requisite fineness of the zinc is produced by pouring the molten metal into a mortar and triturating the same until it solidifies.

Absorbent Power of Red Phosphorus.*—M. Testini announces the fact that the red variety of phosphorus possesses, like porous carbon, the power of absorbing many substances without acting chemically upon them.

Iodine, sulphur, rosaniline, &c., are thus absorbed in quite perceptible quantities.

To show this property by experiment, the author shakes up a quantity of the powdered phosphorus, in the violet colored solution of iodine in bisulphide of carbon; when, if sufficient material is present, the solution becomes colorless. A solution of rosaniline in ether is similarly affected; and may be, in part, recovered again from the phosphorus unchanged, upon treatment with alcohol.

Temperature at Great Depths.—The temperature in the Mt. Cenis Tunnel, at a depth of 5000 feet below the surface, is about 27°C (80.6° Fahr.) This indicates a gradual increase of heat from the surface of nearly 1° Fahr. for every 100 feet of descent; a fact which conforms very well with numerous observations of a similar nature made in various other localities.

Note on a Manganiferous Deposit from Spring Water.—At the last meeting of the British Association for the Advancement of Science, Mr. Emerson Reynolds read a paper† "On the Analysis of a Singular Deposit from Well Water," which recalls an observation of the writer of this communication. The deposit examined by Mr. Reynolds "consisted almost wholly of an oxide of manganese" arising "from the gradual oxidation of manganous carbonate, present in extremely minute proportion in solution in the water." In June of the present year the writer, (then Professor of Chemistry at Delaware College) completed an analysis of the waters from a supposed

* Nature, Oct., 1871.

†See abstract of this paper in *American Chemist*, October 1871, page 127.

mineral spring, situated about one and a half miles northward from the village of Newark, New Castle county, Delaware,—which for upwards of half a century have had wonderful medicinal properties ascribed to them. A quantitative examination showed a wine gallon of the water to contain only 7·08622 grains of dissolved matters, with the following composition: Ferrous carbonate, 0·50532; manganous carbonate, 0·21206; calcic carbonate, 0·95749; magnesic carbonate, 0·28742; potassic carbonate, 0·86402; potassic sulphate, 0·61573; potassium chloride, 0·06484; sodium chloride, 0·74893; alumina, a trace; silicic acid, 1·37050; organic matters, 1·42542 = 7·05173 grains.

The relatively large amount of manganous carbonate (amounting to 3 per cent. of the whole dissolved matters) appeared to be somewhat of a novelty, and led to the examination of the deposit from the spring. This deposit accumulates rapidly, is of a dark brown color, and when carefully dried at 100°C. contains 32.84 per cent. of manganic oxide, the remaining constituents being chiefly ferric oxide, organic matters, and water, with small amounts of calcic and magnesic carbonates. The spring issues from augitic rocks. The valuable therapeutic properties locally ascribed to the water must be regarded as imaginative rather than actual, unless indeed, the manganous carbonate is of some importance in such direction.

CHAS. P. WILLIAMS, *Director Mo. School of Mines.*
Missouri School of Mines, Rolla, Mo., Oct. 20, 1871.

Phosphorescence.—The phosphorescent substance in fishes is announced by M. Pauceri to be the fat; and its cause is declared to be the slow oxidation of the fat in contact with the air, the permeability of the skin allowing this process to go on without difficulty. The phenomenon ceases as soon as decomposition begins; it is equally prevented by the presence of fresh water or carbonic acid, while oxygen, it is said, intensifies it.

To Clean Vessels of Petroleum.*—Thin milk of lime is recommended by Dr. Stolba as an excellent material for cleaning glass or other vessels which have been used for keeping this oil. The lime forms with the oil an emulsion, and renders it easy, especially by the addition of a slight quantity of chloride of lime, to remove even the smell of petroleum. By applying the lime warm, its action is hastened.

* Dingler's Jour., July, 1871.

Thallium.*—In order to preserve the bright metallic appearance of the rare metal thallium, which oxidizes in the air with great readiness, a number of methods have been suggested, but all, it appears, were unsatisfactory. Some years ago, Böttger proposed the use of a concentrated solution of grape sugar, and he now announces that his suggestion has turned out well. The sole objection to this fluid being the formation, in the course of time, of a slimy sediment, from which the solution becomes turbid. The author, in searching about for a fluid which should be free from this objection, announces now that he has found that distilled water answers the purpose perfectly. It is necessary, however, that the water should first be thoroughly boiled to free it from air, and allowed to cool in a closed vessel.

He informs us that a mass of thallium fused beneath cyanide of potassium, and preserved under the conditions named, lost nothing of its brilliant lustre, even after three years.

A New Reagent for Ammonia.† Dr. Lex communicates a new test for ammonia which is asserted to be extremely delicate.

If a solution suspected of containing this substance is treated with phenol, and chloride of lime is subsequently added, a green coloration will appear; even where the quantity of ammonia is extremely slight, the reaction will ensue after the lapse of a few minutes.

Edible Earth.‡—Some of our readers may recall an item with the above title from Prof. C. W.C. Fuchs, which appeared some time since in this Journal,§ from which it appeared that the Javanese and the miners in some parts of Würtemberg were addicted to the curious habit of earth-eating. In relation to this subject, Dr. Göbel furnishes us now with the information that the inhabitants of a small village on the White Sea make use of an earth as an addition to flour in bread making. Its composition corresponds generally to that given in Prof. Fuchs' analysis.

The same author gives likewise the composition of an earth used by the natives of the district of Kirman in Persia for the same purpose.

This material consists mainly of salts of soda, and is readily soluble in weak acids. Since the inhabitants bake bread from flour

* Deutsch Indus. Zeit. 1870, p. 392.

† Ber. d. Deutschen Chem. Ges. iv, 809.

‡ Bul. Acad. St. Petersburg, from *Chem. News*, xxiv, 206.

§ See Vol. lx, 155.

which is fermented so as to become sour, the addition thereto of some of the powder of the mineral substance here alluded to, will have the effect of a kind of baking powder, and at the same time neutralize the acid.

New Products from the Oxidation of Carbon.*—A most important investigation from Prof. Schultze has just been announced, upon the products obtained in the direct oxidation of carbon with permanganic acid in alkaline solution. Besides oxalic and other acids, which were thus obtained in considerable quantity, the savant just named has succeeded in obtaining one which he called provisionally "anthraconic" acid, but which he at the time suspected, and subsequently, with the aid of Drs. Carstanjen and Baeyer, proved to be identical with mellithic acid. The importance of this splendid discovery to theoretical science will be duly appreciated by the laborers in the field of organic chemistry, and at the meeting of the scientific association at which it was announced, it was received with enthusiasm; while as the pioneer research in a field now opened for future fruitful discovery, its value to applied chemistry can hardly be overestimated.

The mellithic acid was obtained from various forms of carbon (amongst which was the graphite), and yielded, on distillation with soda-lime, benzol, and this, upon nitration and subsequent reduction, gave aniline.

A New Mode of Forming Perfect Crystals.—Prof. Schultze states that by the use of a gelatinizing liquid as a solvent, crystals of various substances may be obtained completely formed. In substantiation, a number of fine crystals of sugar, borax, etc., were shown, which had been grown in suspension in gelatin and other solutions.

* Ber. d. Deutschen Chem. Gesell., iv, 802.

Editorial Correspondence.

EXPERIMENTS ON THE EXPLOSION OF STEAM BOILERS.

To the Editor of the Journal of the Franklin Institute.

DEAR SIR.—Herewith please find a copy of the circular issued by the United Railroad Companies of New Jersey, explaining the nature of the experiments on the explosion of steam boilers as proposed by Mr. Francis B. Stevens. If possible please have the substance of it inserted in the next issue of the Journal with this letter. On Monday last, Nov. 20th, Mr. Stevens sent a messenger from New York with a letter to me, requesting that the Franklin Institute might be represented among those who would meet to see the experiments on Wednesday the 22d inst. He says: "As your Institute was the first, many years ago, to originate experiments of this nature, I know that I can, with propriety, ask this favor." The short notice prevented many of our members, who were informed, from accepting the invitation. The experiments tried on Wednesday were of the most interesting character, and will, no doubt, be fully reported in the Journal when you receive the necessary date from Prof. Thurston; but a few words should be said by those of us who were present, as to the general character of the experiments. In the first place, the preparations show with what an amount of energy and determination Mr. Stevens has gone into the work. He informed us that in two months time from the date of the appropriations, he had taken all the boilers described in the circular to the barren shore of Sandy Hook, through the open sea in stormy weather, and put them in place ready for the experiments. The first impression made upon my mind upon seeing the boilers in place was one of astonishment, at the amount of work done in so short a time. The boilers were all connected by feed pipes, and pipes to convey the pressure to the various pressure gauges used. The manner of arranging the various boilers and the care taken in the minute detail of connection show a method that cannot be too highly commended. That anything that is worth doing at all is worth doing well, is a good maxim, and this seems to be Mr. Stevens' idea in preparing for the scene of destruction which must follow the bursting of any, or all of these boilers; with no needless expenditure there seems to have been nothing neglected to secure the knowledge of results. That such experiments must be attended with danger no one can deny, but Mr.

Stevens seems to have thoroughly informed himself by careful preliminary experiments, carried to the limit of safety as indicated by hydraulic tests of the particulars of each boiler, as relates to time required to steam up, and consequently of the time when it should be advisable for the men employed to go away from the strained shells. To me there seemed something sad in the thought that these boilers, which had done such long and faithful service, were now to be torn asunder in the cause of science, and to end their existence in showing to the observant engineers what was to be learned by their destruction.

The first boiler experimented upon was No. 3, a return flue boiler of the same form as that of the Westfield, but smaller. This, intended to carry 40 lbs. of steam when new, now stood, after thirteen years use, the pressure of 93 lbs. without bursting; the many cracks in all parts of the shell under this strain show how uniform was the strength of the whole, and speak well for the design and workmanship. That it would not burst, seemed to be the prevailing opinion of all who saw it steamed up, as I know that in most cases the cracks increase in size in proportion to the pressure, thus forming safety valves for the pent up steam, and do not always permit an explosion to ensue. It would have been interesting to have seen what would have been the result had a large safety valve been suddenly opened, which would have been equivalent to the starting of an engine.

In the actual bursting afterwards of the slab described as No. 6, 6 feet long, 4 feet wide and 5 inches thick, stayed in the same manner as the water leg of the Westfield boiler, a curious phenomena was noticed. On the inside of the disrupted sheets, were found markings as regular as if made on a lathe, of curved lines, surrounding and starting as it were from every stay bolt. These markings were so distinct as to be capable of being transferred to paper pressed upon them. That these were lines of strain seemed to be the prevailing opinion of those who saw them. Hoping soon to see the reported results of the experiments which are to be continued from day to day, I feel sure that I express the opinion of Mr. Jacob Naylor, Mr. Henderson and yourself, who were with me on Wednesday, that the experiments promised much valuable coroboration, if not actual addition, to our knowledge on this subject.

Respectfully,

COLEMAN SELLERS,

Pres't. Franklin Institute.

Civil and Mechanical Engineering.

INTEROCEANIC COMMUNICATION ACROSS CENTRAL AMERICA.

By Prof. J. E. Nourse, U. S. N.

(Continued from page 309.)

The apathy or, at times, virulent opposition of Spain to the improvement of the transit has been noted; the apathy of British authorities and capitalists has been, at times, loudly complained of by their own people.

The latest record against them is in the history of the Panama route. In 1845, Capt. Liot, of the British Navy, Colonial Sup't. of the Royal Mail Steam Packet Company, visited the isthmus to secure the construction of "a Macadamized carriage-road, tram-road, or a railroad, from Portobelo to Panama." The transit was still in its miserable state. "Merchandise was conveyed on mules, or on men's shoulders." The Company claimed the honor of having restored this communication across the isthmus,—a line almost abandoned since Spanish rule ceased. A canal was not now proposed; a railroad, even, was not seriously contemplated; but a Macadamized road was recommended, as the only project "likely, for half a century, to pay," California not as yet being opened.

The government of New Grenada highly favored the plan. Capt. Liot made a favorable report. The British Consul fully sustained him. But the home authorities proved wholly unwilling to afford any such "guarantees or immunities as might secure the Transit Company against undue risk." It was admitted that "stirring events in Mexico and California, and the impulsive agency of American enterprise, were hastening the solution of the question of interoceanic communication." But nothing could be done in England. While Liot's Report was getting into print, and the English were getting quite sure that no improved route would pay, three Americans—Chauncey, Stephens and Aspinwall—foreclosed the case by determining that the Panama Railroad should be built. Admiral Fitzroy said of it, "United States men only" could construct it. He had before urged his countrymen to remember that "the resources of the world were at the command of British capital." (Liots "Panama, Nicaragua

and Tehuantepec," London, 1849; Fitzroy on the Great Isthmus, Jour. R. Geog. Soc., Vol. 23.)

The conclusions to be derived from our narratives thus far seem to be fairly these:

I. From the earliest discovery of America, the riches of Asia stimulated strong desire for an easy transit; the problem has secured an historic grandeur; the search for an open westward passage lay along the whole continent from Alaska to Cape Horn. Even Virginia was explored: "The South Sea," says Bancroft, "was considered the ocean path to every kind of wealth. * * With singular ignorance of the progress of geographical knowledge, it had been expressly enjoined on the Virginia colonists to seek a communication with the South Sea by ascending some stream which flowed from the northwest. The *Chickahominy* was such a stream (!) Capt. John Smith, in ascending it, was captured by the Indians." (Bancroft, vol. i, p. 130.)

It is well known that the great Humboldt, on his visit to the New World, devoted a part of his labors to the investigation of some of the principal routes. His tabulated statement of these will be given in our next.

Three and a half centuries, indeed, have heard the yet unanswered whispers of the Secret of the Strait.

II. The selfish policy of old Spain—part of the policy of the age—suffered all improvement to be forgotten. They would have gladly passed their own galleons only through a natural strait, if it could have been discovered. But neither themselves, nor any other people, company or adventurer were to do anything to open up or improve a transit.

III. The New Republics have been more favorably disposed for improvement. They have been dilatory, inefficient, and incapable of effecting much themselves; but they have not been so blind as to fail to appreciate a route. They have been at times disappointed by the fault of others. In 1846, General Mosquera, President of New Grenada, issued favorable decrees, under the sanction of the National Congress, offering $3000 per annum to a foreign engineer (of Ponts et Chaussees), as superintendent, and providing a fund for his use for the improvement of the national roads. And they said, then, singularly enough: "After ten years of fruitless *expectation*, the govern-

ment is once more in a position to treat with parties who may desire to undertake the formation of an efficient line of communication between the Atlantic and Pacific Oceans."

IV. The English had until our day the best opportunities for opening up improvement.

They had for a long time vast capital uncalled for at home. They had and have the trade of western South America in their hands. A good transit, at any time, would have quickly trebled this as well as their Asiatic traffic.

V. Explorations in later times have not been infrequent, but singularly *unfortunate*. Generally they have not been properly equipped. They have not reached the field at the proper season to give time for thorough explorations before the floods of the rainy season utterly debarred their progress. Little permanent or hopeful result, therefore, has as yet been reached, except it be that certain routes (the fabled Raspadura, or such routes as would require four-mile tunnels) are known as impracticable. Time and money need not be wasted on them. Others—or rather districts in which others may yet be hopefully looked to—have, to this day, been but *partially* examined.

The Tehuantepec and Nicaraguan lines are known to be too long; harbors are wanting for them, and cannot be supplied. The Honduras and Costa Rican routes are those of known great elevations; the Panama route has no sufficient feeder necessary for a canal; the Atrato has no advantages of harbors or of shortness. *Darien* remains, inviting true exploration. This it has never had. Reconnoissances have been made; guesses have been framed; men have talked of crossing it. It remains to-day in the hands of the aborigines who freed themselves from Spanish rule a century ago; a land of mystery.

One of the strangest records in the history of our day stands against this same region of Darien; it is this: that three expeditions, representing the three most powerful commercial nations of the world and the three most interested in discovering this route, should find themselves about the same time in this Darien for the purpose of exploration, and yet should accomplish almost literally *nothing*.

This is the history of the visit to the Pacific side by the Virago, under Capt. Prevost, R. N., and to the Atlantic by the Scorpion and Espiegle; and, about the same time, by the Chimére, under the French Lieutenant Jaureguiberry; and the United States corvette

Cyane, under the ill-fated Strain. It is not, however, injustice to any of these to say, that lack of proper foresight and undue rivalry and haste to cross first, sufficiently account for this failure. It is consolation to know that this history, though seemingly the failure of three great nations, proves nothing against the desirableness of a well-executed, *thorough* survey. This, as all from Humboldt to Malte Brun have agreed in saying, should be on the Atlantic side chiefly, and of the whole cordillera.

What has been learned of the two routes explored by the U. S. Government in 1870 and in 1871, will be referred to under those routes, in geographical order.

Glance now, on the other side, at the *difficulties* of such explorations. Give full credit to the explorers—ardent, sanguine, prudent, energetic, enduring, yet unsuccessful. Fitzroy's sketch of the chief difficulties in the enterprise yet remains true.

Look, for a moment, at some of these difficulties, natural, political and financial:

I. The great isthmus bears upon its face an almost unbroken cordillera; Gisborne says an *entirely* unbroken chain.

There is a marked contrast between the American isthmus and the Isthmus of Suez. America here is a mountain ridge. Africa is an arid, sandy, longitudinal depression, of which more than one-half is on a level with or below the Red Sea and the Mediterranean. To cross Suez is to encounter scarcely an elevation of fifty feet. To cross Central America is to find elevations, in Honduras two thousand nine hundred feet, or in Panama—the line of the lowest level as yet found—elevations from two hundred and seventy to four hundred and fifty feet. The summit ridge on the Panama Railroad is two hundred and eighty-seven feet above the mean tide level of the Atlantic.

If the two requisites for an easy route are, that there be but a reasonable elevation to be overcome, and that a good harbor be found at each end of the line, the isthmus seems to present this singularly formidable obstacle, that these two requisites cannot, so far, be discovered on the same line. If, for example, Caledonia Bay on the Atlantic, or Realejo on the Pacific, be a good harbor, over a large part of the intervening line the cordillera stretches her forbidding frown. Or, if a comparatively lowland route be thought open, by way of

Nicaragua, Panama or the Atrato, the route fails in the absolute requirements of a harbor. Greytown harbor has almost disappeared by the sitting up of the coast; the Atrato has insuperable bars; but the stormy seasons of the northers and of other fearful gales, demand a roadstead.

II. Suppose a practicable route to be found; that is to say, that, during the thorough survey which is still urged by the true advocates of a canal, the labors of the explorer shall permit him at last to show a veritable line, still the difficulties in the construction of a work must prove very formidable. They would bear yet more heavily on a canal than on a railroad. Some of them are:

1. Excessive wetness, owing to torrents of rain and continual showers between the periods of incessant downfall. This must be a serious impediment to the construction and preservation of the solid works of the canal.

2. Malaria, especially near the confluence of fresh and salt water under the influence of the tropical sun. It is by no means pleasant to observe how many adventurers, even as late as the French in 1861—'64, have advanced but a little way, when forced to return by the loss of men and their own breaking down. It is not only, however, that during the visits of explorers or rapid transit of passengers their health will be affected; we are to consider the lives of those permanently occupied in the construction of the work. Many an adventurous spirit must find life afterwards to be a burden, as did Strain, or be worn out by pestilential influences of the swamps, as was Stephens.

"Unquestionably," says Fitzroy, "the climate is a most formidable obstacle. There is no doubt that rain prevails about two-thirds of the year, even on the higher grounds of Darien, while it is no less certain that in the Gulf of San Miguel (where mangrove jungles abound, low muddy shores, and the great fall of tide exposes extensive mud banks) there is a continued succession of rains, more or less heavy, except during short intervals. Examine any travelers's accounts, read their narratives; they themselves bear witness to the undeniable fact, although in *general* terms they may say there is not *so much* rain, and it is not *so* unhealthy as has been supposed.

"It is possible that the great rise of tide on the south side of the isthmus may tend to purify the air on its shores, and this effect in such a place as San Miguel Gulf may be very beneficial."

3. The season in which extensive work can be done is lamentably short. Three or four months in the year—from December to April—is all that can be depended on in Darien; something more of the year in the more northern parts. Even the explorer has been driven off, as were Bourdiol and many others, leaving their work little more than begun.

This and other unfavorable points here named are, of course, well known to the travelers of the day. To the surprise of the writer, however, citizens and even officers have been met with apparently forgetful—if cognizant—of the isthmus season.

4. One more natural difficulty to be noted is the liability of the greater part of Central America to volcanic eruptions. Panama and Darien are comparatively free. Nicaragua commemorates her exposure by her State seal, the device on which is the picture of five active volcanoes.

III. The countless political revolutions in the States are a serious bar to effort. The best conceited plans and treaties have been suddenly cancelled by a revolution displacing the high contracting party before the ink was dry on the instrument of writing they had signed.

III. These natural and political hindrances themselves create the financial obstacles. Capital is always sensitive. Investments to be made in a distant enterprise, require a happy freedom from the most serious obstacles. The history of some schemes on the isthmus has been about this: Some few able capitalists become interested to carry forward an idea of so much worth as this canalization. They send a party of explorers who, coming late, stay long enough in the field to have their energy cooled by the climate and by apprehension of the natives,—a fear shown *now* by Com. Selfridge's experience to be groundless. They *think* they have surely seen a great depression of the mountain chain. They make a favorable report. They are succeeded by a second party, under tacit instructions to fall in with the route already formed. These get up a handsome report, with plates, maps and estimates for the company. Sufficient stock is taken. Then the "concession" is sold out to a third party, and the matter ends.

This kills, for a time at least, all efforts for a vigorous, thorough survey. "It has become difficult," says Mr. Bates, the Sec. R. Geo. Soc., "to awaken interest in the enterprise." The same must be

apprehended in our own country, if appropriations and labors of Government expeditions prove fruitless. And yet the true and successful time *will* come.

ON THE FLOW OF WATER IN RIVERS AND CANALS.

By J. Farrand Henry, Ph. B.

(Continued from page 328.)

Capt. Boileau, in discussing the effect of the wind upon the surface of the water in experimental canals, says:* "In the geometric lines representing M. Hennocque's observations, when a strong down stream wind was blowing, and also in two of those of M. De Fontaine, it is very remarkable that those lines, after being curved to a certain point, as if to give the maximum velocity below the surface, are inflected in the contrary direction under the influence of the wind, so as to bring the maximum to the surface." Thus, this effect of the sides is apparent even when a strong wind is blowing with the current, and carrying the maximum to the surface.

In other observations the data are seldom given with as much detail as those of Darcy and Bazin. But such as are at present accessible are as follows:

Dubuat. An experimental canal, about one foot deep, gave the maximum velocity from $\frac{1}{6}$ to $\frac{1}{8}$ below the surface.

Boileau. In an experimental canal, about two feet wide and one foot deep, it was from $\frac{1}{4}$ to $\frac{1}{5}$ the depth below the surface.

Hennocque. On an arm of the Rhine, where the depth was about eight feet, the maximum was about one-fifth the depth below the surface.

Baumgarten, on the canal du Rhone au Rhin, where it was about 45 feet wide, found the maximum below the surface, except for about three feet in the middle of the stream.

On the Garonne, Baumgarten observed the maximum to be generally at the surface; but in one section it was below for a portion of the river about 325 feet wide near the middle.

De Fontaine found the maximum on the Rhine, where it was about five feet deep, to be at the surface.

Racourt, in his velocity measurements on the Neva, found the maximum at the surface. The river at one point was 900 feet wide and 63 feet deep.

* Mesure des eaux courantes, page 311.

All these observations confirm the law deduced from the Darcy and Bazin observations, that when water is flowing in a channel with vertical sides, the maximum velocity will be found below the surface, when the depth is greater than one-fourth the width; and when the depth is less in proportion to the width, it will be at or near the surface; also if the sides of the channel are sloping, the depth may be one-third or one-half the width before the maximum leaves the surface.

In the Mississippi, the new theories of flowing water place the maximum velocity three-tenths of the depth below the surface.

As these observations are the only ones which do not conform to the general law, it will be well to examine them; particularly as the authors say:* "The method of conducting the field work and of computing the results is given in great detail, in order that it may be seen what degree of confidence the conclusions hereafter to be drawn from this material are entitled to."

On page 230 is the following: "As floats are compelled to pass through nearly the same paths when starting from a fixed station, and are consequently unaffected by the change in velocity due to the difference in distance from the banks, the principle was adopted of depending entirely upon the elaborated sets of observations from anchored boats. All the observations in each set being thus confined to nearly the same vertical plane, one great cause of error was practically eliminated. From the position of the boat, found by triangulation, the recorded gauge reading, and the known depths of the different parts of the river section, the depth of water in each vertical place of passage was readily determined."

It appears from this that the method of observing floats, so elaborately described on page 225,† was not used at all, at least for the determination of the velocities on which the formulæ are based; (although they say "that system was adopted for the observations both of 1851 and 1858,") but merely the time of passage of floats between the two stations was observed.

This, of course, greatly simplified the work; but they were obliged to assume that "floats starting from a fixed station are compelled to pass through nearly the same plane." How erroneous this assumption is has already been shown, and, therefore, in place of eliminat-

* Humphrey and Abbott's Report, page 221.

† *Ante*, page 225.

ing any of the errors of float measurement, it merely adds another to that list.

"It was evident that some combination of curves was necessary to reconcile discrepancies of observations. The first method adopted was to combine all curves where neither the depth of water nor velocity of river varied materially. * * The resulting mean curves are exhibited on figure XI, the numbers being shown in the following tables."* Let us first examine these tables. Here are recorded 2170 floats, of which 233 are interpolations, 1937 floats being observed.

On page 246 other similar tables are given, which were used for testing the formulæ deduced from the first set. These contain 447 floats, of which 109 are interpolated, making in all 2275 floats used in the computations.

In Appendix D, the velocities as determined at all of the stations on the river are given, and the number of floats there recorded exceeds 36000.

During the first year at Carrolton there were nearly 6500 floats passed, noted as at all depths; and though the remainder are given as at or near the surface, yet, as in these tables observations at several other places are recorded, we cannot be far wrong in assuming that there were at least 10,000 floats passed at all depths; especially as at Carrolton alone the work lasted over a year, and this amount would give an average of only fifty floats a day for 200 working days. Whether, if more than 2275 of these floats had been taken, the resulting curves and formulæ would have been changed, cannot be told; but probably these were considered to be the only reliable observations.

These floats give the velocity at 297 points on 69 verticals; and the means of the nine tables or groups in which they are arranged show the maximum velocity to be generally much below the surface, the resulting curves having a decided flexure toward the surface, as well as toward the bottom.

In Table VII these verticals are taken separately, the depth of the maximum velocity and the number of floats passed at that depth being given for each vertical.

The number of verticals in the first set of tables or series is 39, and in 23 of them the maximum velocity is at the surface or one-tenth of the depth below; in the second series, nearly half the verticals give the maximum near the bottom.

* Humphrey and Abbot's Report, page 230.

TABLE VII.

Depth.	First Series.		Second Series.		Total.	
	No. of Verticals	No. of Floats	No. of Verticals	No. of Floats.	No. of Verticals	No. of Floats
0	12	63	8	17	20	80
1-10th	11	46	1	2	12	48
2-10ths	5	33			5	33
3-10ths	3	20	1	3	5	23
4-10ths			4	9	4	9
5-10ths	2	22	1	3	3	25
6-10ths	5	30	1	3	6	33
7-10ths			6	18	6	18
8-10ths			6	15	6	15
9-10ths			2	5	2	5
Bottom	1	8			1	8
Sums	39	222	30	75	69	297

But, if we throw out those verticals where the maximum is at mid-depth and below, which must certainly be erroneous, there remain in the first series 31 verticals, of which 23, or ($\frac{3}{4}$) three-fourths have the maximum at the surface or one-tenth the depth below; and in the second series 14 verticals, 9 of which—two-thirds—show the maximum at or near the surface.

One of the verticals in the first series gives the maximum at the bottom, or rather one foot *below* the bottom, as it is recorded.

Thus, nearly all of these selected observations follow the general law deduced from the European observations; though the means of the groups in which they are arranged do not.

In 1869, very careful observations were made on the St. Clair and Niagara rivers, to determine, if possible, the law of change in the velocities near the surface and bottom. In the measurement of the velocities near the surface, the meter was suspended off the side of the boat, on a pole long enough to remove it from the retarding influence of the boat. The means of the observations near the surface are given in Table VIII.

These velocities are plotted in Figures 7 and 8, being represented by the circumscribed dots.

A free hand curve, shown by the full line, was drawn through as many of the points as possible; and the ordinates of this curve are given in the table.

TABLE VIII.

Depth below the Surface.	St. Clair River.		Niagara River.	
	Observed Velocities.	Ordinates of Curve.	Observed Velocities.	Ordinates of Curve.
Surface		0		0
1	3·607	0·025	2·924	0·015
2	3·651	0·040	2·943	0·035
3	3·651	0·055	2·965	0·045
4	3·662	0·065	2·956	0·057
5	3·689	0·075	2·985	0·070
6	3·676	0·070		
7	3·672	0·065		
8	3·644	0·055		
9	3·646	0·035		
10	3·601	0·020	2·989	—0·005
No. of Verticals	47		45	

In these rivers, as the table shows, the maximum velocity is about five feet below the surface, though the difference between it and the surface velocity is quite small. The mean surface velocity in these rivers was nearly three miles an hour; but in the St. Lawrence, where the current is only about one mile an hour, the surface velocity was often considerably greater than that five feet below; although, unfortunately, too few observations of the surface velocity were made to give it exactly. Thus, these great rivers connecting the lakes agree with the general law heretofore given; the slight retardation at the surface being probably due to the friction of the air. Therefore, in large rivers, we may consider the surface velocity in a calm time to be nearly the same as the maximum; while in narrow canals, especially when their sides are vertical, it may be considerably less.

One curious thing in regard to this descent of the maximum velocity was discovered by MM. Darcy and Bazin in their experiments.* This is shown in Table IX.

The covered canal was a tube, 0·80 m. wide and 0·50 m. high in the first case, and 0·48 m. wide and 0·30 m. high in the second. In both tubes the maximum velocity was found at the center. The cover was then removed from the tubes, and the water allowed to run through with a depth as nearly as possible equal to one-half the height of the tube.

Of these experiments M. Bazin says: "The half of the discharge of the first tube is 0·309 m. The open canal only discharges 0·307 m.;

* Récherches Hydrauliques, pages 176, 177.

but it will be noticed that the surface of the current did not quite rise to the center of the tube, being 0·042 m. below it. If we take this into account, we shall find, all the proportions being the same, that the discharge of the canal slightly exceeds that of the tube."

TABLE IX.

	Canal 0·80 m. wide		Canal 0·48 m. wide	
	Covered.	Open.	Covered.	Open.
	Meters.	Meters.	Meters.	Meters.
Height or depth of water	0·50	0·2458	0·30	0·1513
Inclination	0·00427	0·00430	0·00627	0·00600
Discharge	0·618	0·307	0·191	0·093
Maximum velocity	1·826	1·859	1 634	1·615
Mean do.	1·545	1·561	1·326	1·282
Maximum below surface on center vertical	⅙ depth	⅙ depth	⅙ depth	0
Do. do. side vertical		½ depth		½ depth

And of the second set of experiments he says: "The half of the discharge of the tube is 0·0955 m., or a little greater than that of the canal. The surface of the water in the latter rises above the axis of the tube 0·0013 m. Taking this into account, and also the difference in the inclination, we shall find an excess in the discharge of the tube, very small, it is true, but a result precisely the opposite of that before obtained. * * The comparison of the preceding results appears to demonstrate that in a calm time the resistance of the air has very little influence on the discharge."

But, notwithstanding the very small difference between the discharge of the tube and the canal, the maximum velocity in the first experiment descends from the axis of the tube to one-sixth the depth below the surface on the center vertical, and to one-half the depth on the verticals near the side.

M. Racourt measured the velocity of the Neva where it was covered with ice, thus forming a tube 900 feet wide and 63 feet deep. The observations were made by cutting holes in the ice, and letting down a ship's log.

The following were the measurements on the deepest vertical:

Velocity near the surface, . . . 1·91 feet per second.
" a little below the center, . . 2·58 "
" near the bottom, . . . 1·67 "

The maximum was found a little below the center of the deepest vertical; while in summer—when the cover was removed—it rose to

the surface. These experiments confirm the conclusions drawn from the observations on small canals, that the *locus* of the maximum velocity is due in part only to the resistance of the air, but mainly to the form of the channel in which the water is flowing.

(To be continued.)

BELTING FACTS AND FIGURES.

By J. H. Cooper.

(Continued from page 322.)

Belting.—"A leather strap or belt an inch wide will sustain 1000 lbs. before breaking. I have, therefore, taken 8 per cent. of the breaking weight, or 80 lbs. to the inch, or about 400 feet to the H. P., as a tension that will not materially injure the leather for a long period by overstraining or stretching. This is used for single belts—main drivers only. A double belt will give one-third more equally well.

"As regards the velocity of belts, this subject admits of a wide margin. Ordinarily, counter-belts, where the centers are not more than 12 feet apart, will require 1000 ft. to H. P. per minute, and card and loom belts from 2000 to 3000 to H. P. per minute. When at the Nashua Co's mills I ran a 20 inch single belt 7200 ft. per minute, from a 14 ft. diam. to a 4 ft diam. pulley, which ran successfully on the 14 ft. diam., but the centrifugal force on the 4 ft. diam. pulley caused it to jump or fly from the surface and run a little uneven, owing to the uneven weight and thickness of the leather.

"I think it would have run well on a 6 ft. diam. pulley. When it was running 6000 ft. per minute it ran very satisfactorily indeed, and I could not have asked to run it better. From this experiment, I have come to the conclusion that 6000 ft. is as fast as a belt should run when the pulley is not over 4 ft. diam. Taking this as a basis of calculation, a 10 ft. pulley may run a belt 10,000 ft. per minute with safety. It is, however, seldom in practice that we should use such quick speed. Some three weeks since I commenced running a single belt 5400 ft. per minute,—the smaller pulley being about 4 ft. diam., which gives excellent satisfaction. I know of no definite rule for running belting; everything depends upon surrounding circumstances.

"A horizontal belt, running on not less than a 7 ft. diam. pulley, 50 ft. from centre to centre, and working side at bottom, will run well with 400 ft. to a H. P., the slack being taken up by its own

weight. The same belt, at an angle of 45°, will require 500 ft. to the H. P., and with a vertical belt it will be almost impossible to run it any length of time without a binder, (which of all things we most dread in a mill). I will now mention one law of belting that may not be known to you all, *i. e.*, the hug or adhesion is as the square of the number of degrees which it covers on the pulley, or in other words, a belt that covers two-thirds of the circumference of a pulley, requires four times the power to make it slip as it does when it covers one-third of the same pulley.

"Belts, like gears, have a pitch line, or a circumference of uniform motion. This circumference is within the thickness of the belt, and must be considered, if pulleys differ much in diameter and you must get a required speed.

"Owing to the slip, elasticity and thickness of the belt, the circumference of the driven seldom runs as fast as the driver. With two pulleys of equal diameters, one may be made to run twice as fast as the other without slipping, if you use an elastic belt of india rubber.

I simply mention this to show the effect of elasticity in belts. As the power of a belt is as its velocity, it is well to run it as fast as possible to avoid lateral pressure, and consequently friction of the shaft."

Pulleys.—"One of the greatest objections to the fast running of shafting and belts is the want of pulleys properly constructed. My experience leads me to the conclusion that it is not safe to run a cast iron pulley 4 ft. diam., 400 rev. per minute., owing to the unequal shrinkage of castings in cooling and other imperfections. Running slow, the centrifugal force has but little effect; but as the centrifugal force is as the square of the velocity, it is not so easily overcome in rapid motions.

"If you make the rim of the pulley thicker, the centrifugal force increases with the thickness, and consequently nothing is gained by the extra iron. I have, therefore, substituted white pine felloes made of one inch boards, breaking joints for the rim, built on cast iron hubs and arms. The centrifugal force of material is as the specific gravity, and the specific gravity of cast-iron is thirteen times that of pine, hence the centrifugal force must be thirteen times greater; but the tensile strength of cast-iron is only two to one of that of pine, therefore the rim of a pulley made of white pine felloes will sustain from four to six times the centrifugal force of a rim made of cast-iron;

that is, the same diameter with white pine felloes will run more than double the velocity without being torn asunder. It is less likely to be broken by jar or blow, and is less than half the weight, and of course takes less power to run it. I have run a pulley made in this way, 16 ft. diam., 4 ft. wide, 90 rev. per minute for 18 months. I have just started another, 17 ft. diam., 62 inches wide, 100 rev. per minute, driving on to one made the same way 4 ft. diam. and running 425 rev. per minute. Both of these are working well. I am fully convinced that with quick shafting, *wood* must take the place of *cast-iron* for the rims of pulleys 3 ft. diam. and above.

"No. 2 section of Lawrence Manufacturing Co. has been running with gears, shafting, pulleys and belts, conforming as nearly as possible to the above rules, and is driving the shafting for 38,000 spindles (throstle, ring and mule) with the same amount of power as it formerly required for 19,000 spindles."—*Daniel Hussey, Esq., Lowell, Mass., from Proceedings of N. E. Cotton Manufacturer's Association, No.* 10.

NOTES ON SPRINGS.

By J. H. Cooper.

(Concluded from page 253.)

STEEL SPRINGS.

Rule 1*st.*—"To find elasticity of a given steel-plate spring: Breadth of plate in inches multiplied by cube of the thickness in one-sixteenth inch, and by number of plates; divide cube of span in inches by product so found, and multiply by 1·66. Result, equal elasticity in one-sixteenth of an inch per ton of load."

Rule 2*d.*—"To find span due to a given elasticity, and number and size of plate: Multiply elasticity in sixteenths per ton, by breadth of plate in inches, and divide by cube of the thickness in inches and by the number of plates; divide by 1·66, and find cube root of the quotient. Result, equal span in inches."

Rule 3*d.*—"To find number of plates due to a given elasticity, span, and size of plates: Multiply the cube of the span in inches by 1·66; multiply the elasticity in sixteenths by the breadth of the plate in inches, and by the cube of the thickness in sixteenths; divide the former product by the latter. The quotient is the number of plates."

Rule 4*th.*—"To find working strength of a given steel-plate spring: Multiply the breadth of plate in inches by the square of the thickness

in sixteenths, and by the number of plates; multiply also the working span in inches by 11·3; divide the former product by the latter. Result, equal working strength in tons burden."

Rule 5th.—"To find span due to a given strength and number, and size of plate: Multiply the breadth of plate in inches by the square of the thickness in sixteenths, and by the number of plates; multiply also the strength in tons by 11·3; divide the former product by the latter. Result, equal working span in inches."

Rule 6th.—"To find the number of plates due to a given strength, span, and size of plate: Multiply the strength in tons by span in inches, and divide by 11·3; multiply also the breadth of plate in inches by the square of the thickness in sixteenths; divide the former product by the latter. Result, equal number of plates."

"The span is that due to the form of the spring loaded. Extra thick plates must be replaced by an equivalent number of plates of the ruling thickness, before applying the rule. To find this, multiply the number of extra plates by the square of their thickness, and divide by the square of the ruling thickness; conversely the number of plates of the ruling thickness to be removed for a given number of extra plates, may be found in the same way."

"*Rubber car springs*, to resist actual compression and bulging, should be used in layers with intervening division plates; if used in bulk, should have retaining rings to prevent bursting.

The duration of spring depends, of course, on its quality. Pure rubber is of little use; it must be vulcanized. It is adulterated with plumbago, though in best springs as much as 40 per cent. foreign substances are used—principally sulphide of lead and soapstone."

Four inch diameter rubber car springs weigh 15·7 cubic inches per lb.; 7 inch diameter 16·75 cubic inches per lb. and 11 inches diameter springs, 17·57 cubic inches per lb.; whereof pure rubber weighs 28·8 cubic inches per lb.—*W. G. Hamilton, from Useful Information for Railway Men.*

"*Rule for the strength of Locomotive Springs.*

S = span of spring, in inches.
B = breadth of plate, in inches.
T = thickness of plates, in sixteenths of an inch.
N = number of plates.
L = safe load on spring, in tons."

$$L = \frac{B\ T^2\ N}{11{\cdot}3\ S} \qquad N = \frac{11{\cdot}3\ SL}{B\ T^2.}$$

—*Molesworth's Pocket Book for Engineers, London,* 1865.

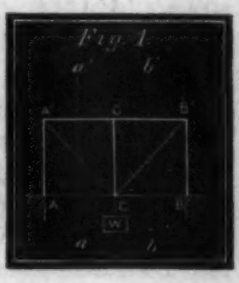
A SHORT THEORY OF THE TRUSS.

By J. P. Frizell, C.E.

A homogeneous beam, supported at its ends, resists the bending tendency of its own and extraneous weights by a strain of compression upon its upper fibres and of extension upon the lower. One of the first properties which the mind perceives in studying these internal forces, is, that the strain upon any fibre is effective in proportion to its distance from the centre, or, in more exact language, to its distance from a horizontal line through the centre of gravity of the section under consideration. This fact suggests the idea of giving increased effectiveness to the tensile and compression strains by placing the portions exposed to them at a considerable distance apart, and confining them immovably, with reference to one another, by ties and pillars or braces. To this combination we apply the term "truss."

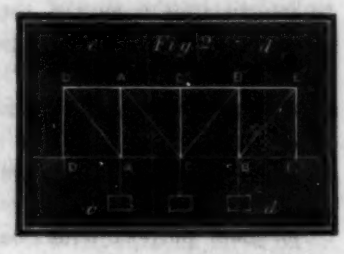
To write an exhaustive treatise upon the truss, and one that might, with confidence, be submitted to the criticism of mathematicians, is a task far beyond the aims and abilities of the present writer. Nevertheless he conceives that a brief exposition of its chief properties by methods suited to the average comprehension will serve a useful purpose.

Let it be required to devise some means of supporting the weight W midway between the two fixed points A and B. Fig. 1 represents one of the methods by which it might be accomplished. Erect the pillars AA′ BB′ upon the fixed points; join their summits by the horizontal pillars or struts C′A′, C′B′, resting upon a third pillar CC′, and let the weight be supported by two ties CA′, CB′. To prevent any lateral motion of the frame, join CA, CB by ties.

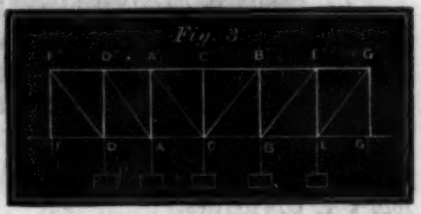


In considering this simple frame, there is no difficulty in estimating the strains upon each of the several members. The pressure acting vertically at C is composed of the extraneous weight W, together with the weight of the pillar C C′, and half the weight of the struts C′A′, C′B′ and ties CA, CB; or, drawing the vertical lines *aa′*, *bb′* through the middle of each panel, the two ties CA′, CB′ sustain the weight lying between these lines. The tensile strain upon each tie is equal to one-half this weight multiplied by the secant of the angle

CA′ A. The compressive strain upon each strut C′A′ C′B′ is equal to the tensile strain upon the tie multiplied by the sine of the angle CA′ A, or equal to the horizontal component of that tension. Each of the pillars AA′, BB′ sustains one-half the weight W and one-half the weight of the structure, not including its own weight. Each of the fixed points A, B supports half the weight of the frame and its load W.



Now conceive each of the points of support to be removed a distance AD = BE = CA = CB farther from the centre. Erect the pillars EE′ DD′. Join D′A′, B′E′ by struts AD′ BE′, AD, BE by ties. Suspend additional weights equal to W from A and B. The portion AA′ B′B with its extraneous weights is now supported in precisely the same manner as the weight at C in the former case. Drawing *cc′*, *dd′* vertically through the centres of the panels AD, BE, the two ties AD′, BE′ sustain the weight between these lines, and each tie is under a tensile strain equal to one-half this weight multiplied by the secant of CB′B = BE′E. The pressure upon the struts C′A′ C′B′ is now increased by that on A′D′ and B′E′ respectively. The tension on CA, CB is equal to the pressure on A′D′, B′E′.



Repeating the process of removing the supporting points and supplying the additional parts and weights, we find as before, that each of the ties DF′, EG′ sustains half the weight lying between their centres, and its tension is equal to this weight multiplied by the secant of CB′B = EG′G. Each of the pillars FF′, GG′ supports half the weight lying between them. The pressures on C′B′ and B′E′ are increased by that on E′G′. The tension on CB is increased by that on BE.

We may pursue this process of removing the points of support till we form a truss of any desired length, loaded with any desired load, and the following general properties of the uniformly loaded truss will become apparent :

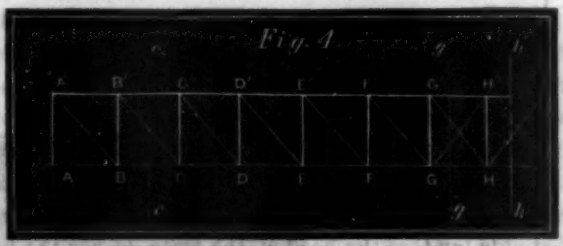
1. Each tie supports the weight lying between its centre and the centre of the truss, and is exposed to a tensile strain equal to the weight supported, multiplied by the secant of the angle between the tie and pillar.

2. Each pillar supports the weight lying between itself and the centre of the truss.

3. The pressure upon the upper chord at any point is equal to the sum of the tensions upon all the ties between that point and the pier, multiplied by the sine of the angle between the tie and pillar, or equal to the sum of the horizontal components of these tensions.

4. The tension upon the lower chord at any point is equal to the pressure upon the upper chord between the same two consecutive ties.

A truss constructed as shown in fig. 3 has all the bracing necessary to insure its stability with respect to a uniformly distributed load. No rational purpose would be served by ties joining the points C′B, B′E, E′G, that is, by what are usually termed counter ties. An increase of the load would bring no additional strain upon such ties, but would tend to loosen them if already screwed tight. The points connected by such ties tend to approach one another under the strain of a uniformly distributed load.



Let us now consider the case of an unequally distributed load. Suppose A to be one of the supports and the portion of the truss next A to be loaded, the rest of the truss being without load. Let A represent the pressure on the pier. *Any tie, as CB′, sustains a vertical pressure equal to A less the weight between A and *c*, the line *cc′* being drawn through the middle of the panel BC. That is, the

* The portion of the truss to the left of *cc′* is acted upon by three vertical forces: 1. Its own weight, acting downwards. 2. The force arising from its connection with the portion to the right of *cc′*, also acting downwards. 3. The pressure, A, acting upwards. As these forces are in equilibrium, the second is equal to the difference between the first and third.

portion of the truss to the right of *cc'* tends to separate itself from the portion to the left of it by a force equal to A less the weight A*c*. It is evident that a tie joining BC' would offer no resistance to this tendency.

As we recede from A toward the right, it is evident that we shall come to a point *g*, where the weight between A and *g* is equal to A; that is, where the tendency of the right hand portion to separate itself from the left hand portion is null. Beyond *gg'*, as at *hh'*, the portion to the left of *hh'* tends to separate itself from the portion to the right of it, and this tendency can only be met by ties, as GH', sloping in the opposite direction. The point *g* is sometimes called the neutral point, since the shearing strain at that point is nothing. It may with equal propriety be called the *breaking point*, since the strain upon the upper and lower chord, where it occurs, is a maximum. Thus the compressive strain upon the upper chord at *g'* is the sum of the horizontal components of the strains upon all the ties to the left of *g'*. The ties under strain to the right of *g'* sloping in a contrary direction, their horizontal components do not increase the compressive strain upon the upper chord, but serve in their aggregate to balance the pressure coming from the left of *g'*. For the same reason the tension upon the lower chord has its maximum value at *g'*. It is, therefore, at this point, if anywhere, that rupture would take place under the assumed load.

The breaking point shifts its position with every change in the distribution of the load. Thus, in an unloaded railroad bridge, the breaking point lies at the centre. A train enters the bridge, and the breaking point leaves the centre and approaches the train, reaches its extreme position, returns, passes the centre, reaches its extreme position on that side, and as the head of the train leaves the bridge, the rear not having entered, it returns to the centre. It describes the same cycle of motions while the rear of the train is crossing the bridge.

Since only the ties which descend toward the breaking point can be exposed to any strain, it follows that between the extreme positions of the breaking point a truss must be provided with ties sloping in both directions; outside of these positions it requires only ties descending toward the centre.

It is customary to speak of ties descending toward the centre as principal ties, and those descending from the centre as counter ties. I adopt this nomenclature, although it implies a misunderstanding of the function of what is called the counter tie; as if it served to coun-

teract, or oppose the action of, the principal tie. All ties are principal ties, or under strain, when the breaking point lies on the side toward which they descend, and counter ties, or out of use, in the contrary case.

It is usual to calculate the strength of a truss with reference to a load of uniform intensity applied to the whole or a part of the length of the truss.

Let AB represent a truss, b the breaking point, c the centre, w_1, the test load per linear foot. The maximum value of bc, or the greatest deviation of the breaking point from the centre, occurs when the test load extends from A to b. For, by the supposition, the total weight between A and b is equal to the pressure at A. Now any diminution of the loaded portion, as, for instance, by removing the load from b to b' will diminish the weight between A and b by $w_1 \times bb'$, but will diminish the pressure at A by $w_1 \times bb' \times \frac{B\dot{b} + \frac{1}{2} bb'}{AB}$. That is, it will make the weight between A and b less than the pressure at A, and will consequently move the breaking point nearer the centre. Again, by extending the loaded portion to b'', we increase the pressure at A without increasing the weight between A and b, and consequently move the breaking point nearer the centre. Since, therefore, we can not increase or diminish the length of the loaded part without diminishing the distance of the breaking point from the centre, we conclude that bc represents the maximum value of that distance.

This maximum value of bc may be determined thus:

Let w represent the weight of the truss per linear foot.
" w_1 " " " " " load " "
" a " span in feet.
" x " distance Ab in feet. Then

$$(w+w_1)\,x = \frac{a\,w}{2} + w_1 x \frac{a - \frac{x}{2}}{a} \text{ or } x^2 + 2a\frac{w}{w_1}x = \frac{w}{w_1}a^2, \text{ whence}$$

$$x = \pm a\frac{w}{w_1}\left(\sqrt{1+\frac{w_1}{w}} - 1\right).$$ The negative value of x simply indicates that there is nothing in the conditions of the question to prevent the whole system from lying to the left of A, as well as to the right.

When the extraneous load is equal to the weight of the truss, foot

for foot, we have $x = a(\sqrt{2}-1)$, or, the greatest deviation of the breaking point from the centre is 0·0858a, and the length of the portion requiring counter-ties 0·1716a, or about one-sixth of the span. When the load is twice the weight of the truss, foot for foot, we have $x = \frac{1}{2} a(\sqrt{3}-1) = 0{\cdot}366a$, or, the maximum deviation of the breaking point from the centre is 0·134a. That is, the truss will need counter-ties a little more than one-fourth of its length. *When $w_1 = 0$, $x = \frac{a}{2}$, or the breaking point is at the centre. We may here recapitulate these general properties of the truss under an unequally distributed load:

1. The breaking point is a point which includes between itself and the pier a weight equal to the pressure on the pier.

2. Its maximum deviation from the centre occurs when the portion of the truss between it and the nearest pier is uniformly loaded with the greatest allowable load.

3. To find its maximum deviation from the centre, or least distance from the pier, we divide the maximum load per linear foot by the weight of the truss per linear foot, add one to the quotient and extract the square root of the sum, diminish the root by one, and multiply the result by the product of the span in feet, and the quotient obtained by dividing the weight of the truss per linear foot by the maximum load per linear foot. The result is the least distance of the breaking point from the pier.

The above are true independently of the arrangement of ties and braces. The following are true for the form of truss under consideration, viz: inclined ties and upright pillars. Tie reaching from top of pillar to foot of succeeding pillar.

4. Only that portion of the truss between the extreme positions of the breaking point requires counter ties.

5. Every pillar bears the weight of truss and load between itself and the breaking point.

6. Every tie descending *toward* the breaking point bears the weight of truss and load between the middle of the tie and the break-

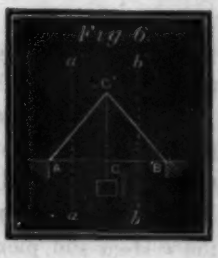
* It might appear at first sight that x becomes infinite or indeterminate when w_1 equals o. Consider w_1 however, as very small with reference to w. In that case, $\sqrt{1+\frac{w_1}{w}}$ is represented by $1+\frac{1}{2}\frac{w_1}{w}$ with a degree of accuracy which increases as w_1 diminishes. When, therefore, w_1 becomes indefinitely small,

$$x = a\frac{w}{w_1}\left(1+\frac{1}{2}\frac{w_1}{w}-1\right) = \frac{a}{2}$$

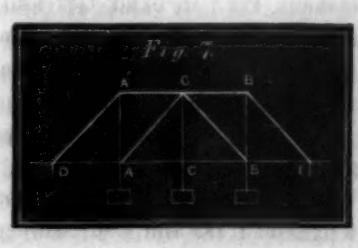
ing point, and is exposed to a tensile strain equal to the weight sustained multiplied by the secant of the angle between tie and pillar. Ties descending *from* the breaking point bear nothing.

7. The compressive strain upon the upper chord at any point is equal to the algebraic sum of the horizontal components of the tensions upon all the ties between the point and either pier.

8. The tensile strain upon the lower chord is equal to the compressive strain upon the upper chord between the same two consecutive *acting* ties.



Let us now return to our original problem of a weight to be supported midway between two fixed points A and B. Fig. 6 shows another mode in which it might be accomplished. Erect the two inclined pillars AC′ BC′ and support the weight by a rod or tie C′C. To prevent any lateral strain upon the supports we must join CA, CB by ties. Draw the two vertical lines *aa′ bb′* through the middle points of the two pillars. Each pillar supports (at its middle point) half the weight included between these lines, and is under a compressive strain equal to the weight sustained multiplied by the secant of the angle BC′C. The tie CC′ supports the weight W and half the weight of the ties CA, CB. The tensile strain upon CA, CB, is equal to the compressive strain on the pillar multiplied by the sine of the angle CC′ A or the horizontal component of that strain, which is also equal to the pressure of the pillars against one another at C′.



Removing the points of support to D and E, adding the ties AA′, BB′, AD, BE, and the pillars A′D, B′E, A′C′, B′C′, and the extraneous weights at A and B, we have the weight of the portion AA′ B′B supported precisely as the weight at C was supported in the former case. Proceeding in the same manner as in the case of upright pillars, we arrive at entirely analogous results for the form of truss shown at fig. 7, which, omitting all intermediate steps by reason of their similarity to what precedes, may be stated thus:

1. Only that portion of the truss between the extreme positions of the breaking point requires counter pillars.

2. Every tie bears the weight of the truss and load between itself and the breaking point.

3. Every pillar ascending *toward* the breaking point bears the weight of truss and load between centre of pillar and breaking point, and is exposed to a compressive strain equal to the weight sustained multiplied by the secant of the angle between tie and pillar. Pillars ascending *from* the breaking point bear nothing.

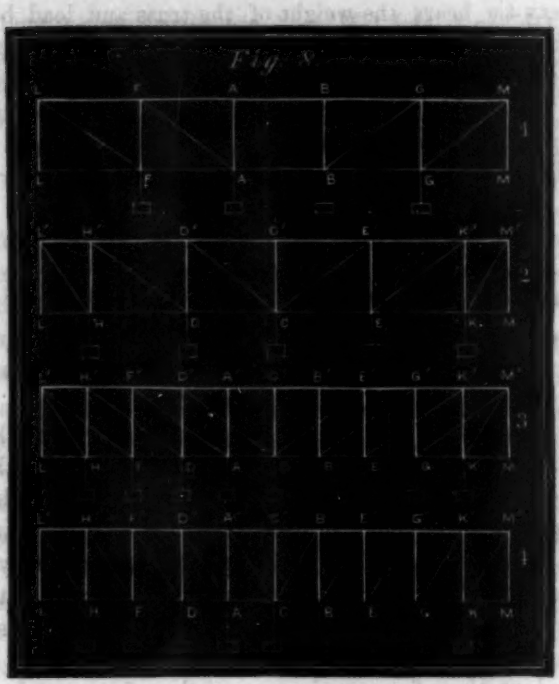
4. The tensile strain upon the lower chord at any point is equal to the algebraic sum of the horizontal components of the compressive strains upon all the pillars between the point and either pier.

5. The compressive strain upon the upper chord is equal to the tensile strain upon the lower chord between the same two consecutive *acting* pillars.

We have thus far considered the simple truss, in which the sloping member passes over only one panel. Let us now consider the system of multiple intersections, in which each sloping member embraces two or more panels. In doing so it will only be necessary to consider the system with upright pillars and sloping ties, since every principle demonstrable of this system has its corresponding truth in the system with inclined pillars and upright ties, arrived at by methods too similar to require repetition.

It is evident that, for any given span, there is a certain height of truss which will give the required strength with the least material; and also, that for any given height of the simple truss, there is a certain width of panel more advantageous than any other. It is, no doubt, within the resources of analysis to determine, in a general manner, the best values of these quantities. Yet a more ready and, all things considered, a shorter method, would be to find them for any given span by trial. For a given span let h be the height and l the width of panel so determined.

Let 1, fig. 8, represent a truss of 5 panels, each equal in width to 2 l. Suppose it to rest at L and M, and to sustain the equal weights W at each of the points F A B G. Let 2 represent another truss with 4 panels of the width 2 l, and 2 of the width l. Let it rest at L and M, and sustain the 5 weights W at H D C E K. The strains upon the several members of these trusses are readily calculated in accordance with the principles already explained, with these exceptions, the reasons for which will appear further on. The pillars composing the upper chord are to be calculated for a length l, and the weight of each of these pillars, as well as that of each of the upright end pillars, is to



be taken at half the weight of a pillar of double its strength. The absence of ties in the central panel at 1 need not embarass the reader. The weight of that panel is sustained in precisely the same manner as the weight at C in 2. Now suppose these two trusses to be united into one, as shown at 3, each pair of top chords, each pair of bottom chords, and each pair of end pillars being consolidated into one. Each of the several members in the compound truss bears the same strain as the corresponding member or members in the simple trusses. We can now compare these strains with those in the simple truss 4, of the same length, the same height, the same number of panels and bearing the same weights.

It will be noticed, first, that each upright pillar in 4, except the end ones, bears sometimes over twice as much as the corresponding pillar in 3; the portion of the weight on each pillar which arises from the weight of other pillars being more than twice as great in 4 as in 3. Any tie, as GK′ in 4, bears more than twice the weight of the corresponding tie GM′ in 3; but the tie GM′, acting at a greater angle with the vertical, will be under more than a corresponding strain. The

strains on GK′ in 4, and GM′ in 3, are respectively proportional to the weight sustained by each tie multiplied by its length. The quantity of metal in each of these ties is respectively proportional to the tension multiplied by the length. It is therefore proportional to the weight sustained multiplied by the square of the length. That is, supposing GK′ to bear twice the weight of GM′.

$$\frac{\text{Weight of GM}'}{\text{Weight of GK}'} = \frac{1}{2}\,\frac{\text{GM}'^2}{\text{GK}'^2} = \frac{1}{2}\,\frac{h^2+4l^2}{h^2+l^2}$$

For the case of $h = 2l$, this expression becomes $\frac{1}{2}\,\frac{2}{1{\cdot}25} = 0{\cdot}80$, or, the weight of GM′ would be 0·8 that of GK′. The end pillars and ties in 3 and 4, each support half the truss and load. The advantage, however, is in favor of 3, since that truss is somewhat lighter than 4. The top and bottom chords in 3 will be somewhat lighter than in 4 for the same reason.

It is evident that the strains in a truss with double intersections may be calculated, with reference to a partial as well as a uniform load, by separating it into two simple trusses.

In an entirely similar manner we may find the strains in a truss in which the sloping member passes over more than two panels.

The preceding examination shows an apparent advantage in the system of multiple intersections. It is only apparent, however, since it involves an assumption whose realization cannot be assured in practice, namely, the assumption that the maximum load with reference to which the truss is proportioned, will always rest equally upon each of the elementary simple trusses. In a railroad bridge, for instance, it is impossible to be certain that the heaviest trains will not sometimes be composed of cars of such length and distance between trucks, that the weight will rest wholly or in great part upon a single system of ties. To give due weight to this fact, it would be necessary to make each elementary truss much more than capable of sustaining one-half, one-third, &c., of the assumed load with suitable margin of security. Similar remarks apply in the case of carriage bridges, and, giving due consideration to this point, it does not appear to me that there is any advantage in the system of multiple intersections.

WOOD-WORKING MACHINERY.

A treatise on its construction and application, with a history of its origin and progress. By J. Richards, M. E.

(Continued from page 318.)

Tenoning Machines.

We give in this number illustrations to a true scale of machines for tenoning, from the designs of the builders, Messrs. Ransome & Co., of London, Eng.

Fig. 1 is what might be termed a transverse cutting machine, the axis of the cutter shaft being at right-angles to the line of the piece to be cut. There are two spindles and two sets of cutters, as seen in the front elevation; the object being to cut from each side of the timber—meeting in the centre so as to prevent splintering.

The weight is one and one-half tons.

Machines of this kind, that are workingon a similar plan, have been very successfully adopted in some of our largest car shops, when the duplication of a large number of pieces is possible. There is, however, but one cutter-head used, which has a vertical instead of horizontal movement. Mr. Isaac Dripps, now Supt. of motive power of the Penna. Central Railroad, was the first to suggest machines of this kind in America, and although they have but a limited range of what we will term adaptation, their importance for special uses has been very fully demonstrated.

Fig. 2 is the standard machine for joined work, as manufactured by Messrs. Ransome & Co. It is of very simple and compact form, with the usual adjustments for spreading the cutter-heads, and is provided with a vertical spindle to carry "scribing" or "coping" cutters, it receives timber five inches thick and eighteen inches wide.

Weight about one ton.

Fig. 3 shows front and side elevations of a compound or double tenoning machine, intended for tenoning both ends of a piece at the same time. One-half the time of operation is saved by this arrangement, while the lengths are more accurate than can be attained with shoulder gauges. Some of the joiner-shops in Chicago have adopted this plan for tenoning sash bars and door framing with quite a saving in time, and the plan has much to recommend it in all cases where a large number of pieces are to be cut without changing.

Weight of the machine is two and one-half tons (English).

The cutter heads for tenoning machines have undergone many modifications; acting transversely to the grain of the wood, they form an exception to most other machines, and need what is known technically as "spurs" to sever the ends of the fibre before the shaving or cut is made.

The cutters also require a kind of shearing action, that is now gained by setting their faces diagonal to the line of the spindle, in which case the faces are planes. At first they were sprung into a kind of curvelinear form to accomplish the same end; in fact some makers yet make them so in England, which is the most to be wondered at when we consider that the effect is quite the same with the straight cutters, which can be made at a much less cost.

ON FEED-WATER TO STEAM BOILERS.

By Ign. Hahn.

Several articles appeared of late in this Journal relating to the use and abuse of steam boilers; the feed-water being a necessary component to it, allow me to make it the subject of this treatise.

Often it may occur that several sources are at disposition from which to draw the water supply; in such cases it is best to consider not only the cost of bringing the water to a certain point, but also the quality of the different waters, *i. e.* how much sediment each will leave on evaporation of equal quantities and what is the chemical composition of such residuum?

At first glance it will be admitted that two boilers of equal dimensions in every particular, of like setting and the same attendance, fired up simultaneously, will show quite a different evaporative power after say eight days service, when the feed-water supplied to the one, leaves for instance but one lb. of sediment for every 1000 gallons of water transferred into steam; while that of the second boiler deposits two lbs. per 1000 gallons.

In other words the boiler with the purer water will, after the eight days operation, require less coal per horse power per hour, than the other boiler; besides, the former will necessitate less stoppages for cleaning or blowing off, and remain a longer time in service before turning unsafe.

But, as already stated, it is not merely the quantity of incrustation. No! also its quality, the chemical composition of the water asks for its share of attention; because when one kind of water contains,

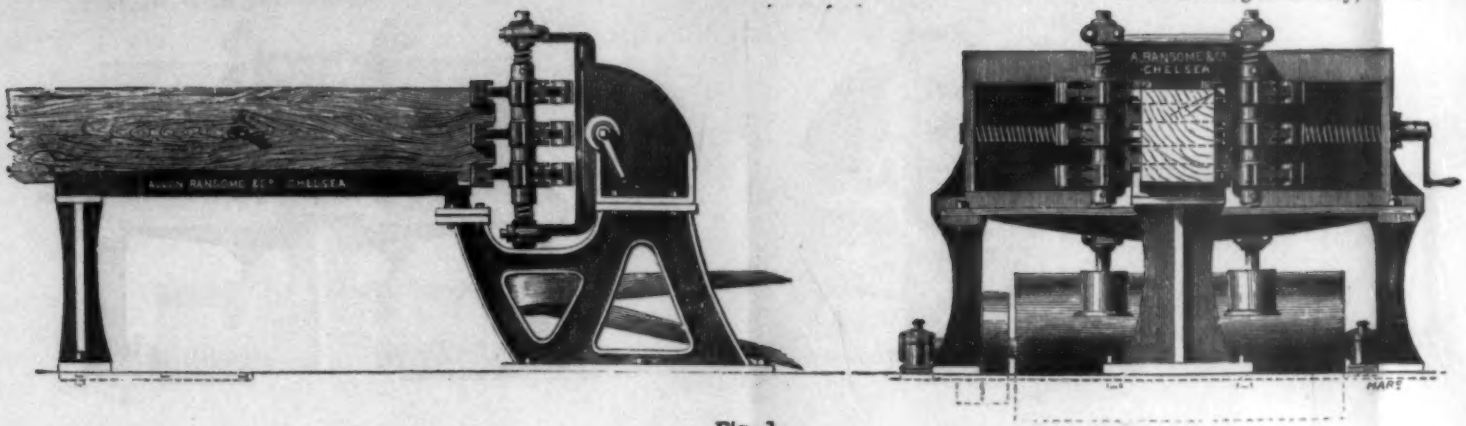


Fig. 1.

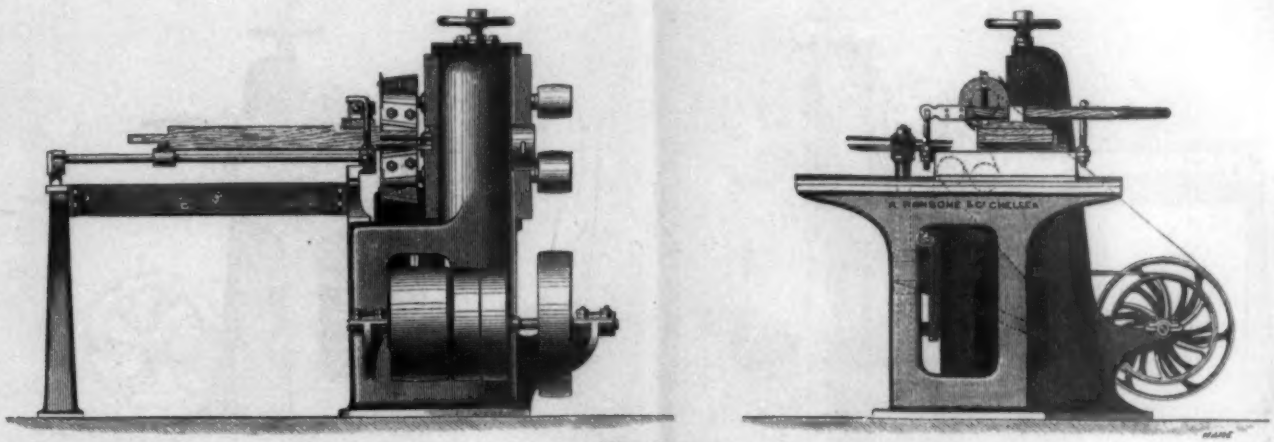


Fig. 2.

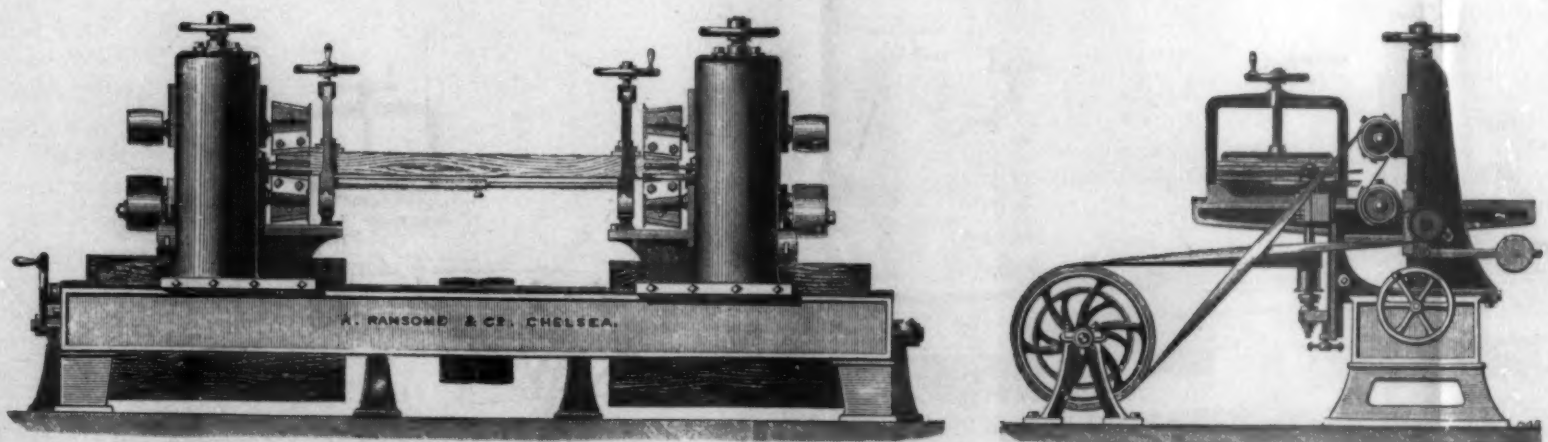


Fig. 3.

IN EVERY 100,000 PARTS (BY WEIGHT) OF THE WATER ARE :

	I.	II.	III.	IV.	V.	VI.
Chloride of Calcium,	0,12762	0,42072	1,64200	0,72260	0,86960	1,02400
Sulphate of Lime,	0,14008	0,59330	0,25500	1,44550	0,17243	0,00680
Carbonate of Lime,	1,81766	4,97969	2,26100			
Carbonate of Magnesia,	0,59724	2,50740	1,28700	0,50438		
Sulphate of Magnesia,					0,32400	0,34500
Sulphate of Iron,				0,53148	0,85500	0,22800
Carbonate of Iron,	0,33690	0,93377	0,10440	0,29043		
Carbonate of Manganese,		trace.				
Carbonate of Zinc,		trace.				
Sulphuric Acid (free),					0,76257	1,
Silica,	0,18500	0,42000			1,95000	0,63000
Organic Matter,	2,00000	1,72328	1,53672	1,52769	16,23740	19,28000
Residuum left on evaporation,	5,1000	11,8550	7,0909	5,2000	21,3000	23,3000
Mechanical impurities likely coming into boiler,	0,3250	1,0500	0,3500	1,1500	1,2300	2,6500
Total Sediment,	5,4250	12,9050	7,4409	6,3500	22,5300	25,9500

for example, free sulphuric acid, the other kind not, then the employment of the latter would be more advisable, even though it may leave 50 or 100 per cent. more of residuum in the boiler.

The case where but one source of water can be relied upon, and that water acting detrimental on the metal of the boiler, will be spoken of hereafter.

As an illustration of the aforesaid the following series of analysis, made by the writer hereof, are entered; in which No. I and II are analyses of water from Franklin Furnace, Sussex Co., N. J., taken July 8, 1871, namely:

No. I from Black Brook, taken at old dam, about half a mile above the mouth of this brook into Wallkill creek (low water stage).

No. II from Wallkill creek, at old dam near Catholic Church (low water stage). No. III, IV, V, VI and VII represent analyses of Roaring Brook water, from or near Scranton, Luzerne Co. Pa., *i. e.*:

No. III taken June 22, 1871, about 600 yards above mouth of Dunmore creek (low water). No. IV taken June 28, 1871, at dam about half a mile below mouth of Dunmore creek (low water). No. V taken June 28, 1871, at dam immediately above upper rolling mill of the Lackawanna Iron and Coal Co. (low water). No VI taken June 28, 1871, at dam just above blast furnaces of the Lackawanna Iron and Coal Co. (low water). No. VII taken July 20, 1871, at same place as No. IV, but at highest water mark known for years past.

The mechanical impurities of the water of analysis No. VII have the following composition:

Silica (sand),	76,0330	per cent.
Sulphuric Acid,	1,5990	"
Protoxide of Iron,	4,8964	"
Alumina,	6,1315	"
Lime,	7,8230	"
Magnesia,	2,9590	"
	99,4419	

(To be continued.)

Mechanics, Physics and Chemistry.

THE CHEMICAL THEORY OF THE VOLTAIC BATTERY.

By J. P. Cooke, Jr.

MAGNETISM.

5. *Polarity.*—Manifold phenomena suggest the inference that, in many cases at least, the molecules of matter are to a greater or less degree polarized, and by this term is simply meant that they possess individually those attributes with which we are all familiar in the masses, and which are peculiarly prominent in the phenomena of magnetism and electricity. A brief summary of the characteristics of the polar condition as thus manifested will greatly facilitate a clear conception of our theory. A common magnet, which is simply a bar of steel magnetically polarized, presents the following characters:

6. *Magnetic Force.*—A magnetized bar has the power of either attracting or repelling any masses of matter which are similarly polarized, whether the polarity be a permanent condition of these bodies or only temporarily induced in them by the presence of the magnet.

7. *Magnetics.*—Only a few materials are susceptible of magnetic polarity to any great degree, and therefore are acted on by ordinary magnets. These are iron in its various conditions, a few of its compounds, together with the two allied metals—nickel and cobalt. Many substances, however, are affected to a slight extent by very powerful electro-magnets.

8. *Magnetic Poles.*—The magnetic virtue appears to reside chiefly at the two extremities of a polarized bar, which are called the poles of the magnet. If the magnetized bar is poised or suspended so that it may turn freely, it always takes a fixed position, in which the line connecting its poles is parallel to what is called the magnetic meridian—a slowly shifting direction at any given point on the earth's surface, but one which in low latitudes is approximately parallel to the geographical meridian. Since the same end of the bar always points north or south, it is evident that there is a difference between the poles, and they are accordingly distinguished as the North and South Poles, taking their names from the poles of the earth, towards which they respectively point.

9. *Similar poles repel each other, and dissimilar poles attract each other*, the force varying directly with the strength of the poles, and inversely with the square of the distance. In all cases

$$F = \frac{M M^1}{D^2}$$

where M and M^1 stand for the strength of the two poles, and D for the distance between them.

If two bar magnets, of equal size and strength, are placed side by side, so that the unlike poles are adjacent, they will tend to neutralize each the other's effect; but, if the like poles are contiguous, the compound bar will exert a greater magnetic force than that of either of the bars alone, thus showing that the two opposite poles are the compliments of each other.

10. *The Poles Inseparable.*—The opposite magnetic poles are always associated on the same magnetized body. They may be very irregularly distributed, as in the familiar phenomena of the multiple poles of a loadstone (fig. 1), or the consecutive poles of a steel bar when not uniformly magnetized (fig. 2); but in all cases the sum of all the north polar forces is exactly equivalent to, and would therefore exactly neutralize, the sum of all the south polar forces.

11. *Magnetic Elements.*—When a magnetized bar is broken, the two halves are each perfect magnets, and the same is true however far the subdivision is carried, the relative position of the new poles thus developed being that indicated in fig. 3. Moreover, when the parts are bound together, the resulting compound bar is found to be uniformly magnetized, with only two opposite poles, like the original magnet. Hence we conclude that the magnetism of the bar, as a whole, is simply the resultant of the polar conditions of its individual molecules, and that they are the ultimate elements of magnetic energy. The polarized structure of the bar may be represented conventionally as in fig. 4.

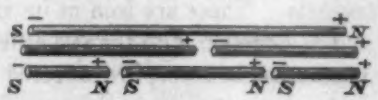


Fig. 3.

Fig. 4.

12. *Magnetic Induction.*—Any body susceptible of magnetic polarity, when brought near a permanent magnet becomes polarized by virtue of the presence of the neighboring poles, or, to use the ordi-

nary phrase, becomes magnetized by induction. The distribution of the polarity depends on various conditions, but in all cases those parts of the body nearest to the poles of the magnet become poles of the opposite kind, and the resulting attraction is merely a phase of the universal principle already stated (§ 9). Magnetic attraction is invariably preceded by the polarization of the body attracted, and hence, in the familiar experiment represented in fig. 5, the key, the nail and the tack are all for the time being as truly magnets as the steel bar which in the given case imparts the polarity.

When magnetism is induced in a homogeneous mass of very soft pure iron, the body instantly acquires the maximum degree of polarity which, under the conditions, the magnet is capable of imparting, and as suddenly loses this polarity the moment it is withdrawn from the sphere of the magnetic influence. Moreover, the distribution of the polarity depends wholly on the size and shape of the mass, and its position with reference to the magnet. It is quite different, however, if the material consists of hardened steel. The body then acquires magnetic polarity only slowly, and, under the same circumstances, never attains it to an equal degree, but the condition, when once acquired, is in a great measure permanent. Furthermore, the magnetism is imperfectly and irregularly distributed, and hence the difficulty of magnetizing uniformly large steel bars, and the various refined methods which have been devised for the purpose.

13. *Coercive Force.*—According to our theory, the induction of magnetism in a steel bar depends on the polarization of the individual molecules; and, since the polarity spreads so slowly and so unevenly through the metal, it is important to bring the magnet as near as possible to every molecule, by rubbing it many times successively over the whole extent of the surface, the slight jars produced by the friction greatly facilitating the change. When a steel bar has thus acquired the maximum degree of polarity it is capable of retaining, it is said to be saturated. Under the influence of a powerful magnet the polarization of such a bar may be frequently raised far above the point of saturation, but the moment the inducing cause is withdrawn all the excess disappears. The loss of polarity, like the assumption of it, must be a molecular effect, and that resistance in each molecule of the steel which, in some unknown way, tends to preserve the polar condition, we call the coercive force. In the molecules of soft iron there is little or no coercive force, and hence in bodies of this mate-

Fig. 1.

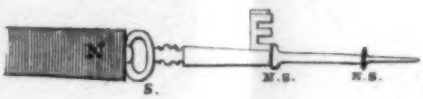


Fig. 5.

Fig. 2.

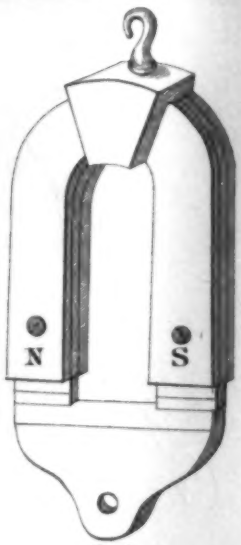


Fig. 6.

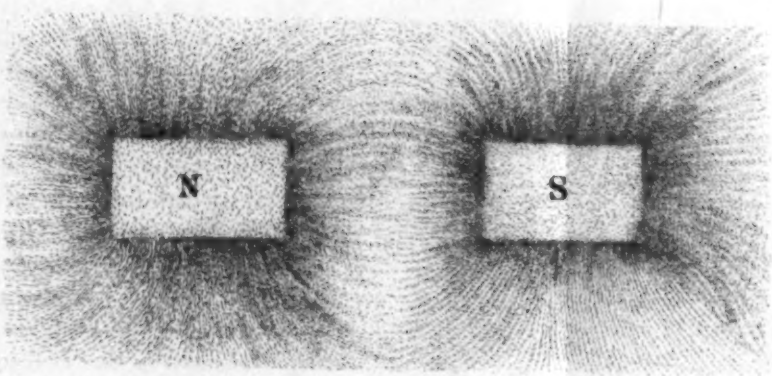


Fig. 7.

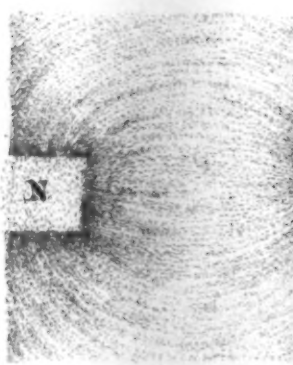


Fig. 8.

Jas M^c Gregan Lith. Phil^a

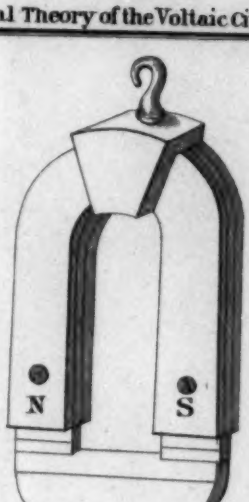


Fig. 6.

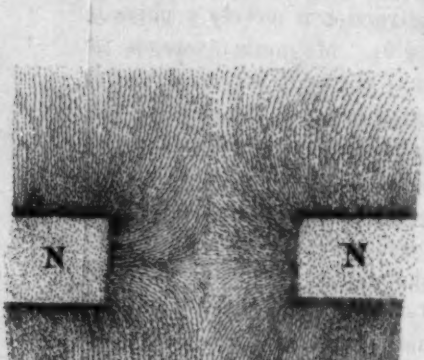
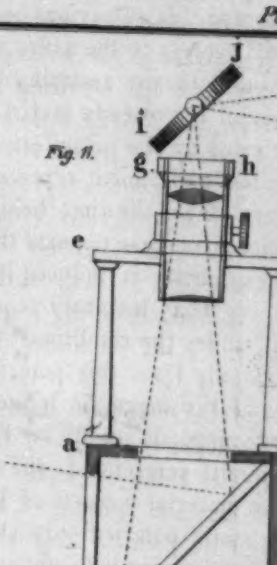


Fig. 9.

Fig. 11.



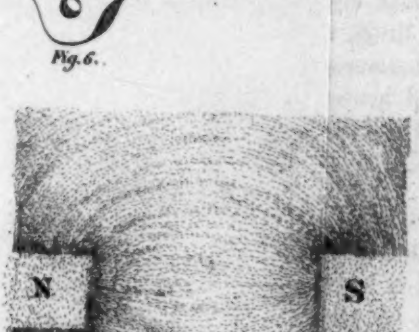


Fig. 8.

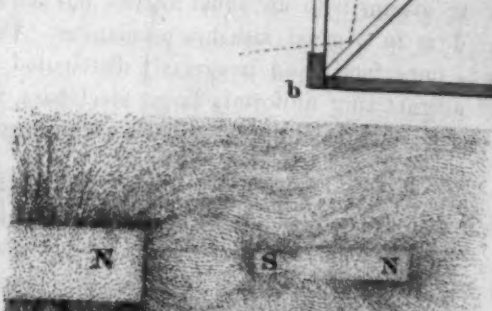
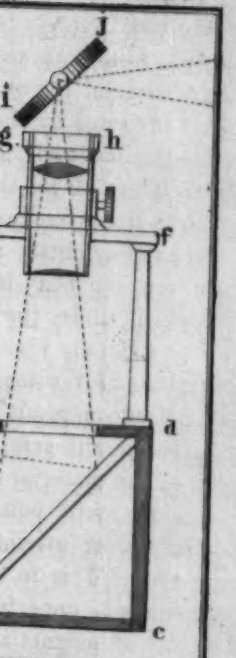


Fig. 10.

Jas Mc Gowan Lith. Phil.a

Plate I.



rial magnetic polarity is always dependent on some external cause; but soft iron readily yields to the magnetic influence, and assumes at once the maximum degree of polarity which the cause is capable of producing. In the molecules of steel, on the other hand, the coercive force is greater the greater the hardness of the metal; but then they assume with proportionally less readiness the polar condition, and the mass becomes, under the same circumstances, less strongly magnetic. In making a magnet, therefore, we have to consider not only how much polarity the metal can retain, but also how much can be imparted to it, and the best result is reached when these opposite conditions are most nearly balanced.

14. *Induction not a Transfer of Energy.*—In the process of magnetizing a steel bar, and in all cases where magnetic polarity is induced, the magnet employed suffers no loss of energy, but, on the contrary, may gain in power by the reaction of the newly polarized material. The effect of the "keeper" on a horse-shoe magnet is a remarkable illustration of this point. A horse-shoe magnet is simply a bar of magnetized steel, so bent that the two poles can be applied to the opposite ends of a much shorter bar of soft iron, which is called the keeper. The two poles, acting in concert on the same soft metal, impart to it a high degree of polarity, and this mass of polarized iron, reacting on the magnet, serves not only to "keep" up its polarized condition, but may even raise it far above the point of saturation of the steel. In all such cases, however, the moment the keeper is withdrawn the magnet falls back to its normal state.

A horse-shoe magnet, with its keeper attached, as in fig. 6, forms what is called a closed magnetic circuit, and, for the reasons already intimated, the force with which the magnet holds the keeper is far greater than twice the force which could be exerted by either pole acting singly.

If we place in contact with a powerful magnetic pole one end of various iron bars of the same quality and size, but different lengths, we shall find that the strength of the pole induced at the opposite end diminishes with the length of the bar. If, next, we take bars of the same quality and length, but different sizes, it will appear that, within certain limits, the pole is the stronger the larger the size of the bar; and, lastly, if we experiment with bars of the same dimensions, but different qualities, it will be seen that the strength of the pole increases with the softness of the metal. The extension of magnetic polarity through an iron bar is merely an effect of induction from

molecule to molecule in a uniform mass of easily polarized material. In this respect it resembles electrical conduction, but it differs from this allied phenomenon in so far as there is none of that transfer of energy along the lines of polarized molecules which chiefly characterizes a current of electricity. As has been stated, many substances are susceptible of magnetic polarity to a slight degree, but only the three metals named above are capable of transmitting the polarity to a marked extent, and this power is greatly diminished by the least impurity, which increases their hardness. Soft iron is by far the best magnetic known, but even in the softest iron bar, when united to the strongest magnetic pole, the effect is insensible at the distance of a few feet. Coercive force is obviously directly opposed to the transmission of magnetic polarity. Heat also lessens the susceptibility to magnetic polarity, and at a high temperature all traces of it disappear.

15. *Diamagnetics.*—In the ordinary experiments on magnetic induction the effect is produced through a layer of air, but the polarizing power of a magnet is exerted equally well through glass, copper, zinc, paper, wood or any other non-magnetic material. It is a familiar experiment to show that a horse-shoe magnet attracts its armature as strongly through a screen of glass or of pasteboard, as through an equal thickness of air. Materials through which magnetic induction is thus exerted without sensibly modifying their condition are called diamagnetics. Through magnetics the polarity is also transmitted, as we have seen, by induction from molecule to molecule; but in such cases we have evidence that the intervening material is throughout in a similar polar state. Moreover, there appears to be an essential difference between magnetic bodies of all kinds and the diamagnetics; for small masses of any diamagnetic material are repelled from either pole of a powerful electro-magnet, while even the feeblest magnetics are attracted under like conditions. So, also, a diamagnetic needle (made of bismuth, antimony or phosphorus, for example), when suspended between the poles of such a magnet, points not to the poles, but places itself on a line perpendicular to this axial direction, a position in which all its parts are at the greatest possible distance from the two equally repellant poles. It is probable, however, that in all cases of magnetic induction the polarity is transmitted step by step through the intervening molecules of the etherial medium with which all space is supposed to be filled, and, if so, we should expect that the material of the diamagnetic would modify to

some extent the result; but our knowledge on these points is as yet very limited.

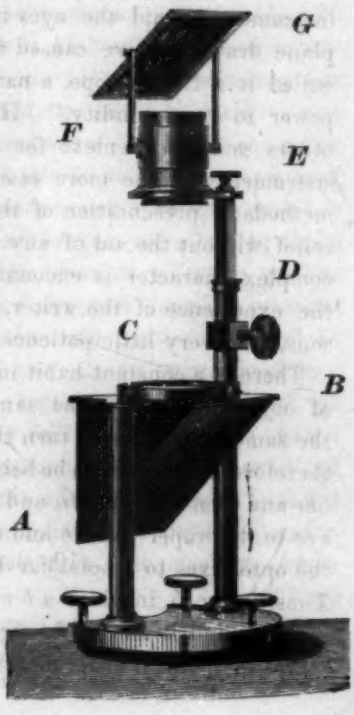
16. *Magnetic Field.*—An advantage is frequently gained by studying the phenomena of magnetic induction from a somewhat different point of view. Every magnetic pole may be regarded as modifying the conditions of the space around it, and the region within the sphere of the polar influence is called the magnetic field. This field, again, may be regarded as crossed by lines of magnetic force, which all centre at the pole. If the pole were a single point, and completely isolated, these lines would all be the radii of the sphere, having its centre at that point; but, as every magnetic pole is a mass of polarized material of a definite size and shape, and is always associated with its opposite, the relations of the lines is far less simple, and the direction at any one point is the resultant of the many polar forces acting at that point. This direction can be determined experimentally by means of a small magnetic needle, which at once assumes the direction of the lines of magnetic force at the point where it is suspended, and the lines in any section of the field can be most distinctly mapped on a plate of glass by means of iron filings sprinkled over its surface. The glass having been so placed above the magnet as to form the required section of the field, it will be found, on tapping the plate, that the particles of iron at once arrange themselves along the lines of force. Each of these particles becomes, in fact, a magnetic needle, which readily turns on the smooth surface of the glass. In fig. 7 we have a representation of the result when the plate rests directly on the two poles of a horse-shoe magnet. When the plate rests on a single pole of a bar magnet the lines diverge as radii from that centre, and by opposing to each other either like or unlike poles, of the same or different strength, various associations of curved lines will be formed, showing what complicated effects may result from the combined action of the two polar forces. The curves shown in fig. 8 were obtained with two equal and unlike poles, and those of fig. 9 with two equal and like poles. Lastly, the effect represented in fig. 10 was obtained by placing near a single pole a small piece of soft iron. The polarity of the small bar and its reaction on the magnet is here distinctly exhibited, and the effect will be more evident if these curves are compared with those at either end of the bar magnet on fig. 7.*

* These phenomena can be most beautifully projected by means of the apparatus exhibited in fig. 11. The wooden box, *a b c d*, is about ten inches cube, and has a circular aperture in front seven and a half inches in diameter, which

receives the tube of the condenser of a magic lantern. A diagonal partition, *b d*, supports a plate glass mirror, placed at an angle of 45° to the front of the box, and measuring nine by six inches. In the cover, *a d*, is inserted a small "light" of plate glass, about six inches square. Above the box is fastened, by four "uprights" at its corners, the board *e f*, which supports a common photographic camera tube (portrait tube, whole plate size). On the upper rim of the tube rests a brass collar, *g h*, to which is fastened the small plane reflector, *i j*. This last should have a good optical surface, but a common plate glass mirror will answer every purpose. The camera tube must bring to a focus on the screen the image of the glass plate, at *a d*, and hence the height of *e f* above *a d* should correspond to the focal length of the tube, which should be provided with rack and pinion for adjustment. The iron filings are sprinkled on the glass plate at *a d* with a small fine sieve, and the surface is then covered with a second plate of thin glass. This is best set in a shallow frame, to prevent it from coming in contact with the lower plate, and also partially coated with black paper, leaving only a circular aperture about three inches in diameter, which cuts off the chromatic edge of the circular image. The magnet is next placed upon the upper plate, and when all is ready the cover of the box is gently tapped. The curves at once take form on the screen, and the effect is almost magical. The same apparatus can evidently be used for projecting a great variety of effects which can only be exhibited on a horizontal plane.

If the condenser of the lantern is at least seven inches in diameter, the simple apparatus just described will be found to give perfect results, and it can be made, by an ordinary mechanic, with materials which the art of photography has rendered very common. It, moreover, has the great advantage of not requiring delicate adjustments. But with a smaller condenser the section of the cone of light made by the glass plate at *a d* would not be sufficiently large. This difficulty, however, can be remedied by using, in place of the glass plate, a plano-convex lens, having the same diameter as the condenser, and a focal length about equal to that of the common tube. A lens so placed forms, indeed, a part of the condenser, and nearly its whole surface can be illuminated; but the several lenses must then be accurately centred, and the construction of the apparatus requires the skill of a philosophical instrument maker. A beautiful instrument of this sort, devised by Professor Henry Morton, of Hoboken, and made by Messrs. Hawkins & Wale, of the same place, is shown in fig. 12.

Fig. 12.



CONTRIBUTIONS TO THE SUBJECT OF BINOCULAR VISION.

By Prof. Chas. F. Himes, Ph. D.

(Continued from page 348.)

PRODUCTION OF STEREOSCOPIC EFFECT BY MEANS OF THE PICTURES, WITHOUT THE AID OF AN INSTRUMENT.

Since pictures can be obtained corresponding to the retinal impressions in all particulars except size, as those of the frustum, fig. 6, it seems reasonable to expect that, if these pictures should be presented to the eyes in such a way that each should view the one proper to it, the conditions essential to relief would also be present, and the object represented should and in fact would consequently appear in relief.

This interesting fact was undoubtedly first established by Professor Wheatstone, in 1838, although much has been written by Sir David Brewster to detract from his credit in this respect. As it is unnatural, or rather contrary to unvarying habit, to look at one point with one eye and at another point, a few inches to the right or left, with the other, as would be necessary when each eye should view the half of the stereograph proper to it, Professor Wheatstone contrived an instrument to aid the eyes in so doing. And since, by its aid, two plane drawings were caused to present an appearance of solidity, he called it a Stereoscope, a name of Greek derivation, indicating this power to "see solidity." His instrument has been supplanted by others more convenient for ordinary use. The explanation of the instrument will be more easily made after the consideration of the methods of presentation of the pictures to the eyes so as to produce relief without the aid of any instrument, In this case a difficulty of complex character is encountered, which can, however, according to the experience of the writer, be very readily overcome by most persons, with very little patience and practice.

There is a constant habit in all individuals, from the earliest notice of objects, to look at the same point of an object with both eyes at the same instant, or to turn the optic axes upon the same point. If, therefore, a stereograph be held up before the eyes, both will look first at one and then at the other, and it will be found difficult to direct the right eye to its proper picture and the left eye to its picture, or to converge the optic axes to a point far beyond the plane of the point observed. Thus, in fig. 5, in which $a\ b\ c\ d$ and $a'\ b'\ c'\ d'$ represent the binocular pictures, and R and L the right and left eye respectively, when L observes the point a of its picture, R turns its axis, by force of habit, to

the same point, instead of to the corresponding point a' of its picture, and, if its axis should be turned to a', the axes would be convergent upon A, beyond. But the violation of this habit does not constitute the whole difficulty. The optic axis is not only directed to the point to be seen distinctly, but there is, at the same time, a focal adjustment of the eye to that point. When an object at the distance of 10 inches appears distinct, another at the distance of 20 inches will appear indistinct. When, therefore, the optic axes are converged upon a point observed, the eyes are unconsciously accommodated for distinct vision at the distance of that point. These consensual movements of convergence of the axes, and accommodation of the eyes to the same point, invariably accompany each other. In the attempt to view the stereograph, so as to obtain appearance of relief, a demand is made to separate these invariably accompanying movements, namely, to adjust the eye to see distinctly the points in the plane of the pictures, and to converge the axes to a point more remote. Thus, in fig. 5, when R is adjusted to a', and L to a, A will be the point of convergence of the axes.

The following method will be found to aid most persons in acquiring the power to dissociate these operations of convergence and adjustment, and to cause the pictures of an ordinary stereograph to combine, to form a clearer, more exquisitely beautiful figure in relief than can be obtained by aid of an instrument: Fix the eyes upon a distant object, in a remote part of the room will answer, and gradually introduce a stereograph about 18 inches from them. Four indistinct pictures will generally make their appearance, since each eye will see the two, but gradually the two interior ones will approach each other, and finally coalesce and produce the appearance of relief; but, although the relief will be unmistakable, there will be an indistinctness about the object, a hazy, atmospheric effect, which in some cases clears up almost instantaneously, and in others more gradually, just as the habit of convergence and adjustment may happen to be overcome suddenly or gradually, and the eyes be permitted to accommodate themselves to the points of the pictures observed whilst the axes remain converged on the remote points.

If, instead of the photographic pictures, two circles, about half an inch in diameter, or two wafers, be substituted, the experiment will generally prove easier, especially if their centres are not more than $2\frac{1}{2}$ inches or less apart. This is probably due to the fact, in the first place, that there would be less about them to invite that involuntary

scrutiny by the eyes, which would render it more difficult to hinder their concert of action, and, in the second place, if the circles were nearer together, the point of convergence of the optic axes would not be as far beyond the pictures, and the difference in distance between the point of convergence and that of accommodation would not be so great, and the violation of the habit consequently not be so decided. For the same reason, by cutting a stereograph apart, and sliding one picture partially over the other, the experiment will often succeed on first trial.

In these cases of *ocular* combination, so called in contradistinction to the instrumental combination, of stereoscopic pictures, it is plain that the angle formed by the optic axes is less than if they were converged to the point of accommodation; or we may say the eyes are rolled outward more than is natural for the distance of the picture observed. There is, however, another *ocular* method, and one which is generally suggested first, and with a preference, namely, by *converging the axes to a point between the eyes and the pictures*, or by squinting. In this case the pictures must be transposed, as will be evident upon inspection of the figure, fig. 7, in which R′ and L′ represent the right and left eye pictures, respectively, of fig. 5 and fig. 6, but in transposed positions. The axis of the right eye, R A′, crosses the axis of the left eye, L A, when the eyes are directed to the corresponding points, A′ and A, of their pictures. In order to use ordinary stereographs in this way they must be cut apart, or drawings similar to fig. 6, which is half the natural size, may be made, in transposed positions. It will be of great assistance, in first attempts at combination by this method, to hold a pencil a short distance, say 10 inches, from the eyes, and whilst looking at it to introduce the stereograph beyond it, and move it back and forth until combination is effected as in the previous case.

In either of these cases the appearance of relief may be explained, not only by saying that proper dissimilar retinal impressions are pro-

duced, as in looking at an object, but on the theory of successive convergency of the optic axes before given, although it may be best to state here that it will be found necessary subsequently to modify somewhat this theory. It is, however, correct enough in its general features to justify its use in this connection.

Thus, in fig. 5, the optic axes form a smaller angle with each other when directed to a and a' than when directed to c and c', just as they do in case of the points A and C of the real object; or, we may say, the eyes are rolled inward when the axes are turned from the points a and a' to the points c and c' of the pictures, just as when turned from the point A to the point C of the object. When the method of convergence to a nearer point is employed, it will be apparent, from fig. 7, that a similar movement of the axes takes place.

If it be conceded that the method of convergence to a nearer point is more readily acquired, as is generally stated, although the writer is not conscious of the difference in his own case, the explanation would seem to lie in the fact that the points of accommodation and convergence more nearly agree in the one case than in the other; for, when the similar points are separated by nearly the interocular distance, the axes in passing through them will be nearly parallel, the point of convergence will be at a great distance; but in the second method the point of convergence will lie between the eyes and the pictures, and therefore much nearer to the latter, whilst the eyes in both cases must be accommodated to the same distance, namely, the plane of the pictures.

But statements are frequently made, in this connection, by some of the best authorities on the subject of Binocular Vision, which are calculated to mislead, and to prevent individuals from enjoying and cultivating the power of combining pictures by the first method. Thus, Sir David Brewster remarks: "The distance in this case must be greatly less than the distance of the eyes, in order that the optic axes, in passing through similar points of the two plane pictures, may meet at a moderate distance beyond them," (p. 83.) "We cannot thus unite figures the distance of whose centre is equal to or exceeds two and a half inches," (p. 97). "It is impossible to obtain by the ocular stereoscope pictures in relief from the beautiful binocular slides which are made in every part of the world for the lenticular stereoscope," (p. 123). In his able and exhaustive papers on Binocular Vision (Silliman's Journal, new series, vols. xx and xxi), Prof. Wm. B. Rogers states: "In this mode of combination, the picture of the ob-

ject proper to the right eye is viewed by that eye, and the picture proper to the left eye by the left, just as with the common stereoscopes. Hence the drawings intended for the latter instruments may be used in this, provided they are so close to one another as to bring the points which are to be united nearer together than the centres of the eyes," (xx, p. 92.) The instrument alluded to consists essentially of a thin strip of wood, with a sliding stage to hold the stereograph whilst viewed by the *unaided* eyes. In his recent "Notes on Light," Professor Tyndall says: "The impression of solidity may be produced in an exceedingly striking manner without any stereoscope at all. Most easily thus:" Here follows a description of an experiment by the second method, and then, without alluding to the first method: "For this experiment the drawings are best made in simple outline, and they may be immensely larger than ordinary stereoscopic drawings." In another place, Sir David Brewster, in speaking of the method first given, remarks: "This tendency to a distant convergency is so rare that I have met with it only in one person." And, without alluding to the dissociation of the axial and focal adjustments of the eyes in connection with this difficulty, he dwells mainly on this fact of distant convergency.

The assumption implied in the foregoing quotations of the necessity of convergence of the optic axes to a positive point, either before or behind the slide, in every case of combination of supplementary pictures, and the consequent limitation of the distance of separation and size of the pictures, seems to originate mainly in the fact that in all cases of vision of an actual object the axes do so converge, and the tendency in writers to locate the resultant solid image at the point of convergence of the optic axes, is perhaps partially due to the same fact. According to the individual experience of the writer, and observation in cases of many others, the optic axes need not necessarily converge to a positive point, but may be parallel or even divergent. In these latter cases there would, of course, be no point of convergence at which to locate the resultant.

It seems, indeed, that the angle of convergence is not a criterion of absolute distance, but that the variation of the angle simply affords the means of comparing distances. The assumption alluded to must be founded either upon the hypothesis that it is impossible to render the optic axes parallel, as would be necessary when the similar points should be the interocular distance from each other, or divergent, as would be the case when the identical points were farther than that dis-

tance from each other, or that when the axes are in either of these positions, and the supplementary pictures presented so that the axes may pass through similar points, no union attended with the usual affect can take place. A difficulty of the first kind would only lie in overcoming an unvarying habit of concert of action of the eyes; for, as before stated, the eyes are capable of an outward movement of about ninety degrees. The second hypothesis would be met by facts to the contrary. Many persons can unite the ordinary binocular slides, even when the pictures on them are three inches apart. In fact, it requires but little practice for most individuals to do so.

The suggestions made to aid in uniting the pictures by looking at a distant object, and introducing the stereograph between it and the eye, could not, of course, apply when the distance between the similar points was greater than the interocular distance. In some cases, when individuals have succeeded in the experiment with the pictures at this distance from each other, it has been after staring at the pictures as if gazing into vacancy, as is done sometimes in resting mind and body, or when the mind is abstracted in thought, and persons and things are stared at but not noticed, the visual organs being inactive. It would seem, therefore, that the state of rest of the optic axes, or the normal state, is one of parallelism, convergence the habit when active. But, after the power has been acquired, as previously directed, it will be found that the pictures, if cut apart, can be gradually separated, and the axes will remain fixed upon the pictures, and follow them, whilst the eyes will retain their accommodation to them. The observer will be unconscious of any change, the whole effect will be unmarred, until a separation of three or even more inches is reached. According to the experience of the writer, in a series of experiments on this subject, after a separation of three inches has been passed in the above experiment a change will be felt, and probably during the first trial all will become confused at three and a half inches, but up to that point the stereoscopic effect will be complete. By practice, however, the power was acquired of uniting the ordinary binocular slides as readily without as with an instrument, and by the method just given experiments made upon the limit of separation of the pictures in his case, and afterward by larger photographic pictures prepared for the purpose. It was found at first that it required effort to overcome three inches, but by prolonged practice at different times the distance was increased to four inches. Exercise of this kind is, however, uselessly severe; further experiments in direction of in-

crease of distance were therefore abandoned, and the limit in course of a few months receded to three and a half inches. After an hour's exertion, as above, great difficulty was experienced in reading; it required conscious effort to prevent the letters from becoming double; the habit of converging the optic axes to the point of accommodation had been so broken that it became necessary to retain them in position by a decided effort. In reading old books, the old-fashioned s's were peculiarly annoying, as they invariably doubled; and when anything unusual or difficult was met with—as, for example, the rendering of a word in translating—the eyes immediately attempted to assist in overcoming it by turning the axes in all directions, causing the words and letters to march and countermarch from right and left. No such effect results from a moderate amount of exercise in using the ordinary stereographs. The weariness of the eyes in all the preceding cases does not seem to be wholly due to the unusual character of the operation, but in part to the fact that the eyes notice many details when supplementary pictures are combined that would otherwise be neglected, perhaps, even in looking at the scene itself. To take an extreme case: a line may appear to be a blemish, in a single picture; it may appear to have no position, by reason of perspective; but, by binocular examination of the two pictures, it may turn out, perhaps, to be a twig, which extends far into the foreground, all other parts of the tree having been cut out; thus there may be dots which turn out to be leaves, and so forth. The eyes, instinctively, as it were, run over and study these exceedingly more multitudinous and minute than pre-Raphaelite details of the photographic representations, they work rapidly and severely, and soon become fatigued.

The bearing of the facts just given upon the theory of binocular vision will be noticed hereafter.

ON THE SOLUBILITY OF SOME FORMS OF PHOSPHATE OF LIME.

By Chas. P. Williams,
(Late Prof. of Chemistry Delaware College.)
Director Missouri School of Mines.

Agricultural experience has long appreciated the difference in the rapidity of action of ground bones on the one hand and of some of the phosphatic guanos on the other. The decided preference given to the first form of phosphate of lime and to the various manipulated guanos and so-called superphosphates is not merely the result of

prejudice, but is in exact accordance with the results obtained by chemical experimentalists who have shown that the solubility of tri-calcic phosphate in water containing carbonic acid is as various as the source from which it is obtained. Bischof has shown* that while one part of apatite will dissolve in 393,000 parts of water saturated with carbonic acid, the artificially prepared basic phosphate requires only 1,102 parts of the same for its solution. As theoretical considerations would appear to indicate that all forms of phosphate of lime must pass to a condition corresponding to that of the artificially precipitated phosphate before assimilation by vegetation, the rapidity of the action of carbonic acid water in rendering phosphates soluble becomes an indication, measurably, of the relative rapidity of their action when applied as fertilizers, and hence of their comparative values in such capacity. Analytical investigations in this direction would seem to be of more immediate practical value than the simple quantitative determination of the constituents of a phosphatic guano or fertilizer, and may yet serve to harmonize some of the apparent discrepancies now obtaining between the results of the laboratory and the farm. Condition of molecular aggregation, determined by solubility in carbonic acid water, is of, at least, equal importance as percentage richness in phosphate of lime, and a rapid and reliable analytical method that may discriminate between such conditions of aggregation in superphosphates is a desideratum alike to chemists, manufacturers and farmers. Justice to all parties concerned makes it imperative that the analyst of an artificial phosphatic guano should distinguish between the original insoluble phosphate of lime and that which has become such by reactions in the article in question, and until this is done the analytical estimations of soluble and insoluble phosphoric acid fall short of furnishing fully reliable data for estimating commercial value. And this is equally true of natural or unmanipulated guanos.

The writer has endeavored to determine the solubility of some of the best known forms of natural tri-calcic phosphate by operating in the following manner: A weighed amount of the finely pulverized material was placed in a measured quantity of water (1000 cubic centimetres), and carbonic acid gas passed through the same for fifty hours at a temperature of 60°—70° Fahr. At the end of this time the liquid

* Elements of Chemical and Physical Geology, Cavendish Edition, Vol. iii, page 27.

was filtered off, nitric acid added to strong acid reaction, the solution concentrated by evaporation and the phosphoric acid separated by means of molybdate of ammonia. The precipitate thus obtained was treated in the usual manner to convert the phosphoric acid into ammonio-magnesic phosphate, care being taken to allow the solutions in which this was forming to stand at least twelve hours before filtration. To avoid loss, the ammonia water used for washing was in all cases made of uniform strength, the amount used in each analysis was measured, in connection with its proper filtrate, and for each 54 cubic centimetres of the liquor, ·001 grm. was added to the weight of the pyrophosphate of magnesia obtained. This correction for the solubility of ammonio-magnesic phosphate in ammoniated water was made in all cases. To render the conditions of the experiments as nearly as possible uniform, the amount of phosphate of lime present was made identical in each case, an analysis of the sample being previously made and a proportionate increase or decrease of amount being taken to correspond to increased or decreased per centage o phosphate of lime.

Experiment No. I.—Apatite from near Perth, Canada, containing 89·27 per cent. $3CaO\ PO_5$. Amount taken, 0·5 grm. Solution gave ·0021 grm. PO_5, equivalent to ·0045 $3CaO\ PO_5$. This amount corresponds to one part of the phosphate of lime of apatite dissolved in 222,222 parts of water saturated with carbonic acid.

Exp. No. II.—Same as in No. I, but the material levigated. Solution gave ·0032 grm. phosphoric acid, equivalent to ·0071 grm. $3CaO\ PO_5$ or one part in 140,840 parts of water saturated with carbonic acid.

Exp. No. III.—Fine ground and pure bone containing 56·78 per cent. of phosphate of lime. Amount taken, 0.733 grm. Solution gave ·0804 grm. phosphoric acid, equivalent to ·1753 grm. phosphate lime. One part of the phosphate of lime in raw bones dissolves in 5698 parts of water saturated with carbonic acid. According to Bischof* fresh ox-bone shavings require 4,610 parts, while Liebig† says only 1,500 parts of carbonic acid water are required.

Exp. No. IV.—To determine whether the organic matter of the bone had any influence on its solubility, a portion of the material used in III was calcined and the organic matters burned off. The ash corresponded to 61·13 per cent. of the original bone and, therefore,

* *Loc. cit.* † Quoted by Bischoff, *loc. cit.*

contained 92.88 per cent. of tri-calcic phosphate. Amount of bone ash taken, ·4805 grm. Solution gave ·0569 grm. phosphoric acid, equivalent to 0·1246 grm. phosphate of lime, or one part in 8029 parts of water saturated with carbonic acid.

Exp. No. V.—A sample of adulterated bone dust of commerce, containing 24·32 per cent. organic and volatile matters, 35·06 per cent. phosphate of lime and 26·13 per cent. of sulphate of soda (salt cake with a small amount of acid sulphate). Amount taken, 1·3157 grms. Solution gave 0·1111 grm. PO_5, equivalent to 0·2426 grm. 3CaO PO_5—or one part in 4,122 parts carbonic acid water. The greater solubility of this sample is, in all probability, due to the formation of some acid phosphate of lime (CaO 2HO PO_5) by the small amount of free sulphuric acid present in the salt cake with which it was adulterated.

Exp. No. VI.—South Carolina phosphate, from the property of the Charleston Mining and Manufacturing Company, containing 57·89 per cent. phosphate of lime. Amount taken, 0·771 grm. Gave PO_5, ·0647 grm., equivalent to ·1413 grm. 3CaO PO_5, or one part in 6983.

Exp. No. VII.—Same as No. VI, but levigated; gave ·1528 grm. 3CaO PO_5, or one part in 6544.

Exp. No. VIII.—Phosphatic guano from Orchilla Island. Containing moisture 12·88 per cent.; organic and volatile matters 14·60 per cent.; 3CaO PO_5, 49.67 per cent.; CaO CO_2, CaO SO_3, sand, clay, etc., 22·76 per cent. Amount taken, ·8970; gave 0.0588 PO_5, equivalent to 0·1248 grm. 3CaO PO_5, or one part in 8009 parts of water saturated with carbonic acid.

Such well-known guanos as the Narassa and Swan Island were not experimented with on account of their larger per centages of phosphates of iron and alumina.

It is not claimed that these results will indicate the absolute degree of solubility of the several materials in the soil when the water is not saturated with carbonic acid and where there are so many modifying and controlling conditions, but they serve to indicate, with some degree of exactness, the relative rapidity of action of the different forms of phosphate of lime, and to show the wide range of solubility of the compound under similar conditions, according to the source from which it is obtained. They, perhaps, rather enhance than decrease the difficulties in the way of discriminating between the chemically precipitated and the original undecomposed phosphates of a

"superphosphate" by throwing suspicion on the absolute exactness of all those methods of separation based upon solubility in weak or organic acids, unless indeed, the operator is acquainted with the raw materials employed by the manufacturer of such "superphosphate." They and all similar experiments, however, fix the great value of this precipitated or "returned" phosphate, by showing how much more rapid it is in its action than is any form of phosphate, to whatever degree of fineness it may be brought by purely mechanical means of subdivision.

Missouri School of Mines, Rolla, Oct. 16*th*, 1871.

ON MR. CROOKES' FURTHER EXPERIMENTS ON PSYCHIC FORCE.

By P. H. Vanderweyde, of New York.

Mr. Crookes gives, in the last number of the *Quarterly Journal of Science*, some "further experiments on psychic force." It turns out now, that when twelve months ago he announced that he "was about to investigate the phenomena of so-called spiritualism," he had already been converted to this delusion, because now he says: "I have been working at the subject for two years, and have found nine or ten different persons who possess psychic power in more or less degree." Consequently, in beginning the investigation, twelve months ago, he discarded the golden rule of Descartes, the first sentence in his philosophy, namely, "In order to arrive at the knowledge of the truth it is our duty to commence with doubting everything." Mr. Crookes did not doubt twelve months ago, but started then with the belief that there are really persons who possess the so-called psychic power, which, as appears from his further elucidations, consists chiefly in increasing the action of gravitation. For instance, he says "objects varying in weight from 25 to 100 lbs. were temporarily influenced in such a manner that I and others present could with difficulty lift them from the floor."

To this I remark that it is a common experiment by so-called psychologists to let people try to lift heavy weights, influencing, at the same time, their mind in such a manner that they imagine the weight has considerably increased. I have myself witnessed exhibitions where strong men declared they could not lift a psychologised weight at all, and I am confident they were honest; but it was not the weight which was psychologised, but their minds and imagination; this was

it which influenced and partially paralyzed their muscular efforts. In all such cases I myself never had the least trouble in lifting the weights, and never found any difference in their gravity, even when a dozen persons testified to the contrary; the simple reason of this is my unflinching faith in the universal law of gravitation and my firm conviction of the absurdity of the hypothesis that these laws could be changed by certain individuals—in short the total impossibility of psychologising my mind. During some such experiment I had the weight suspended from a large balance and placed in equilibrium with a counterpoise, but then no medium ever succeeded in increasing or diminishing it a single ounce. The believers, however, as well known, are ready for such an emergency: my "unbelief disturbed and neutralized the physic force of the medium."

I am indeed surprised that Mr. Crookes experiments exclusively with the unreliable spring balances, which he surely does not use in his chemical investigations. Nothing is easier than to practice deception with the same, for which reason a certain class of traders use them exclusively, and for the same reason their use has been forbidden in some countries. Why does Mr. Crookes not make use of his old honest chemical balance? No doubt he has at least one in his possession much more delicate than the apparatus he describes, which, for the possessor of a good balance, is totally unnecessary. I have one which, when charged with a 100 grammes, will be immediately affected by a milligram and indicate plainly a tenth of this, or one-millionth part of the charge. I have repeatedly challenged psycologists to influence my weights, but they never were able to add one single milligramme or to disturb the equilibrium by the effort of their will, notwithstanding my balance is so sensitive that the animal heat of the body, when sitting long at one side in making a series of weighings, exerts a decided influence.

If the use of the ordinary balance is objected to on the ground that the psychic force would act equally on both weights, I answer that, by Mr. Crookes own showing, this supposed force fluctuates so much that it is exceedingly improbable that it would act with such perfect equality on two distant weights as to show no motion in a sensitive balance. Besides it would be an easy matter to construct a balance of which the beams are so long as to bring the counterpoise beyond the influence of the psychic force; suppose it is not asserted that this influence acts equally all around the medium to any distance.

Mr. Crookes, in confirmation of his views, relates a few cases of similar experiments by Gasparin, Thury and Dr. Hare. If the first two have no more foundation than the latter the testimony is worthless. It shows only a prejudice in Mr. Crookes' mind to attach any value to the report that Dr. Hare communicated the details of his experiments to the American Association for the Advancement of Science at the meeting in August, 1855. I was present at that meeting; it was held in the city of Albany; and I declare that the apparatus was the grossest piece of self-deception one can imagine. The fact was the eminent man had become insane; the meeting would not listen to him, on the ground that his theory of spiritualism was on a religious subject or a delusion, and that in either case it was totally foreign to the field of a scientific association. On the same ground I had several years before often succeeded in keeping spiritualism out of the discussion of some scientific societies in New York. The new names invented for it—biological psychic, cctenic, nervous or vital force—are nothing but attempts to introduce this deception to the attention of scientists, under the pretext that it originates by a new, unknown force.

I close with a few serious objections to the manner of experimenting adopted by Mr. Crookes. When he traces his curves of variable pressure, for instance, he sets the clock going when Mr. Home gives the word, and does not keep it going before Home exerts his pretended influence, in order to see if the same curves are not being produced previously by some other, unnoticed cause. Again, another person put his hand on Mr. Home's hand; this is under pretext to find if Mr. Home exerts pressure, while, in fact, there are now two hands pressing; it is even stated that sometimes his hands and feet were firmly grasped, etc. There is too much interference or rather resistance there; it reminds me of one of the many psychological exhibitions I witnessed in New York, when six strong men were ordered to keep a small table down, which, it was said, would be raised from them by the spirits. They kept it down so powerfully, and in their attempts, continually encouraged by the psychologist, twisted it so from its legs that it broke up into splinters, while the audience wondered at the great force exhibited by the spirits.

Prof. Crookes complains that his testimony is not believed in this case, and his veracity questioned, while he is accustomed to be believed without witnesses. It is surprising that he does not perceive the difference in the two cases; when he published investigations con-

cerning his valuable chemical discoveries, there was nothing contradictory to the experience of any one, but when he comes to relate his experiments on psychic force he makes assertions so contrary to the experience of every man's whole life time, that he reasonably cannot expect to be implicitly believed; he ought to know that the faith of the world in the most eminent scientific men does not go so far as to the belief in their infallability.

I have not investigated this matter for two years, but for twenty-two years, and I am known in this part of the world as one of the strongest opponents to all such delusions. I have very often exposed the trickery and deception, but always declared that I am open to conviction, and have always been ready to be present when experiments in this line were made. As I have but one desire, *to know the truth*, and but one fear, *to believe an error*, I will be the first to acknowledge it when I shall be convinced of the reality of the existence of a psychic force which can influence the gravity of bodies. To do this, however, I do not trust tricky spring balances, but my own chemical balance, my trusted friend, who never in my life deceived me. The microscope and telescope may deceive, the spectroscope may give deep problems to solve, but the balance is the only perfectly truthful and reliable adviser of the scientist. Demonstrate the reality of the psychic force with the balance, Mr. Crookes—measure its intensity in grammes—then we will have something tangible, and the whole scientific world will be convinced of the reality of another force besides the forces already known, and a new subject for research.

NOTES ON THE COLOR OF FLUORESCENT SOLUTIONS.

By Prof. Henry Morton, President of Stevens Institute of Technology.

No. II.

I have recently observed a curious action, which, while it in no respect affects the general conclusions given on page 140 of the present volume of this journal, nor the main observations on which they were founded, throws out one of the corroborative experiments by which I thought that they might be established when a spectroscope was not at hand.

Obtaining some very anomalous results of late, I was led to mistrust the action of the geissler tubes in which the solutions had been examined.

These were of the ordinary kind of jacketed spirals, selected as being nearly identical in size and other particulars.

It had been observed from the first that the internal spiral gave a faint blue fluorescence, which could only be seen on close inspection, and in all cases the tube being but partly filled, it was considered that a light appearing in the part covered by the fluid, many times more bright than that from the uncovered part of the spiral, was sufficient evidence of fluorescence in the liquid.

Late experiments have, however, proved that this was not so. Any liquid, however devoid of fluorescent properties, gives all the appearance of fluorescing in these tubes, and on a little thought the cause of this became clear.

The only fluorescent light that can be seen from the glass of the spiral is that which comes off tangentically from the outer surface, the foreshortening increasing its effect, while, moreover, that emitted radially is masked by the bright electric discharge behind.

In passing from the glass to air, most of this light will suffer total reflection at the outer surface of the glass; but if water or any other liquid is substituted for the air, its greater refracting power (approaching that of glass) will diminish the above-named action, so that much more of the light will reach the eye. The truth of this explanation was supported by the observation that the nearer the index of refraction in the liquid came to that of glass the brighter was the light seen through it.

This fact renders of no account the observations before made on filtered and diluted solution of turmeric, but a fresh observation with the spectroscope on tubes free from fluorescence has fully confirmed my former conclusions as to the true color of fluorescence in this liquid.

No correction need be applied to the case of the description already published in the asphalt solution, but I may add to what was there stated another striking example:

If one of the little geissler tubes containing nitrogen, called "spectrum tubes," be jacketed by means of a perforated cork and a large glass tube, and the jacket filled with pure or non-fluorescent benzine, then illuminating the tube, and with a pipette dropping in that petroleum product called "cosmoline" (a lubricating oil made by E. H. Houghton, of Philadelphia), each drop will appear of a rich blue as it dissolves in the benzine, which soon acquires a magnificent blue fluorescence. Increasing, however, the quantity of cosmoline oil un-

til its color begins to take effect, the tint of the fluorescence gradually changes to a rich green.

By a little care a blue solution may be superposed on a green one in the same tube.

Another semi-solid preparation of cosmoline, which has a very light color, gives a solution with benzine fluorescing of a magnificent blue.

I have this substance now under investigation, and hope soon to be able to make some further observations upon it.*

Returning to the solutions of turmeric, I have found that the fluorescent body in that substance is not its essential oil, nor its brown coloring matter, but either the yellow coloring matter itself, or something so closely allied to it in solubility, that I have thus far been unable to effect any separation.

In connection with this, I am much indebted to Mr. Robt. F. Fairthorne, of Philadelphia, who has aided me greatly in the preparation of the various constituents of turmeric in a state of purity.

In my former paper I mentioned that uranium nitrate in solution gave a very faint fluorescence.

This appearance I now find was due entirely to the above explained action of the tube, and a number of carefully conducted observations now convince me that this substance, while it fluoresces so vividly in the solid state, loses that property entirely when in solution.

I have also found that a saturated solution of acid quinine sulphate had its fluorescence much *increased* by dilution.

Let me remark, in conclusion, that I would by no means assert that all solutions fluoresce blue, but simply that a number already specified, and generally credited with other colors, possess this property.

* Mr. Houghton tells me that "cosmoline" is prepared from crude petroleum, by evaporation *in vacuo* and filtration through animal charcoal only, without any chemical treatment.

Bibliographical Notices.

A Manual of the Principles and Practice of Road Making; comprising the Location, Construction, and Improvement of Roads, Common, Macadam, Paved, Plank, etc., and Railroads. By W. M. Gillespie, LL.D., C.E. Tenth edition, with large addenda. Edited by Cady Staley, A.M., C.E. A. S. Barnes & Company, New York and Chicago. 1871.

We regret our inability to commend the typography and general style of getting up of this book, which is defaced by deficiencies in the printing and final revision of proofs. The first edition—1847, had an extensive circulation, and has been constantly followed by others, until we now have the tenth before us. This commercial success indicates, we think, deserved popularity in a work which supplied an obvious public want by furnishing within moderate compass a useful practical manual of road making, without excluding appropriate mathematical formulæ which assist to explain its rules. The steady circulation of the work has made former editions of it so familiar to our readers interested in road making that they will scarce expect of us an effort to appreciate its merits thoroughly and assign its place in professional literature, but rather a sketch of the additions which have been made. The first edition contained 336 octavo pages; the present one contains 464 pages of the same kind, showing a gain of 128 pages of the original size. Of this increased bulk 20 pages are added to the article on "Plank Roads," which the author considers as "the most valuable improvement since Macadam's, and one superior to his in many localities." "What Railroads Ought to Be," in respect of direction, grades, width of gauge, and other features, gains 8 pages. The comparative advantages of broad and narrow gauge railroads are discussed, and the preference given to the narrow gauge, in order to prevent a "break of gauge," although a gauge of 5½ or 6 feet is considered desirable theoretically; but the "narrow gauge" here alluded to is the gauge of 4 feet 8½ inches, and not the "narrow gauge" as understood in relation to such roads as the Festiniog Railway in Wales, and which has recently much occupied public attention. The "narrow gauge question," in this sense, is not touched upon. "Working Expenses" gains 1 page; the tabular statements cited are of the year 1851. All this makes a gain of 28 pages in the main text, which in other respects stands as in the first edition, except, perhaps, very

slight alterations. The remaining increase of about 100 pages is in the "Appendix," divided into several heads. Here the calculation of "Excavation and Embankment" gains some 30 pages, of which 15 are occupied by a valuable memoir, published in this *Journal*, 1857—1859, and 4 pages of tables from those of Macneil, to whom the writer appears to give preference in this respect. "Location of Roads" gains 6 pages; laying out of "Curves," 24; "Estimation," 4; "Tunnels," 3; "Bridges," 23; "Specifications," 3; "Railroad Resistances," 5; and "Staking out Side Slopes," 3. In our opinion, the work would have been improved, and the claims of the public, after such long patronage, better satisfied, by appropriately disposing of all or most of the matter contained in the addenda within the body of the work. Our readers, we hope, will understand that our strictures are not meant to apply to the labors of the author, but solely to what seems to us to be inferior mechanical execution and insufficient editorial arrangement and revision, for a standard professional Manual.

ERRATA *in Professor Young's "Spectroscopic Notes" (published in the last number of this journal).*

P. 149, lines 9 and 10 from top, for reflected read deflected.

P. 355, line 12 from bottom, for co-efficient of dispersion, read co-efficient of deviation.

P. 356, line 1 from top, for $D = m\,n\,s\,\partial f'.\,d\mu$, read $D = m\,n\,\partial f'.\,d\mu$.

Page 358, line 10 from top, for $A = \frac{l m n \triangle f_1^2}{f}$ read $A = \frac{l\,m\,\triangle f_1^2}{f}$

Page 358, line 13, for $L = \frac{i\,l\,w\,a^2 - f^2 S}{l\,m^2\,n\,\triangle f_1^2 f}$ read $L = \frac{i\,l\,w\,a^2 - f^2 S}{l\,m^2\,\triangle f_1^2 f.}$

P. 358, line 16, for $L' = \frac{i\,w\,a^2}{m^2\,n\,\triangle f}$ 3, read $L' = \frac{i\,w\,a^2}{m^2\,\triangle f^3.}$

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